THE TELEGRAPHIC JOURNAL.

Vol. I.—No. 3.

ASPECTS OF INTERNATIONAL TELEGRAPHY DURING WAR-TIME.

Though Peace and War are obliged, for the sake of appearances, to sustain a sort of formal opposition to each other, they are nevertheless such excellent friends in reality that it is difficult to know how one could ever survive the other. War has afforded practice for doctors, has procured news for editors, has led to discoveries in nosology, has opened vast regions for commerce, and has furnished with facts and statistics airy speculators of all sorts. Peace, on the other hand, is always willing to see her inventions and discoveries adapted for purposes of destruction to human life. Her improved knowledge of shipbuilding, of mechanics generally, of chemistry, of optics, are all at the service of War. But is her old friend as welcome to the last adaptation of one of her most vital discoveries—the electric telegraph? Peace will have to draw the line somewhere, and it looks as if it might be here. For while the construction of systems of telegraphy would be of the greatest use for warlike purposes, their destruction would be equally an object in the same cause; and such a policy as that would be so diametrically opposed to the interests of Peace as to make it imperative to consider the aspects of international telegraphy in time of War.

The telegraph, by land or sea, is *prima facie* one of the first considerations of either belligerent. To know the enemy's position, to ascertain his numerical force, to order arms and ammunition from remote emporiums, are all processes open to electric help. But then these wires measure tracts of territory, these cables stretch some thousand leagues; hence a hostile army on march or a cruising man-of-war can render the stations thus connected useless, just by severing the wires or cables at single points. Again, what havoc might ensue if one of the belligerents captured a telegraphic station belonging to the other side, and succeeded in keeping that fact to himself! The insight that might be obtained into the working of the opposite side, the misleading advice and false communications that could be sent, might turn the fortune of a war. This possibility would in itself be a source of danger, which, coupled with the necessary liability to destruction, goes far towards limiting the warlike advantages to be derived from the systems of telegraphy with which the world is enriched. Of course the extent to which the new apparatus might be employed by belligerents to further their conflict would be strictly confined to those wires and cables either under their complete control or possessed in common; but it is also evident that those systems shared by either belligerent with a neutral power would be in danger of destruction.

Consideration for the safety of such valuable property, and of such an indispensable accessory to our modern civilisation, would induce neutral nations to be on the alert to save the telegraph, which is as remarkable for its precarious situation as for its invaluable use; and neutrals will have a clear right to insist on the preservation of their own cables or wires. But they can only demand that respect for property by observing certain conditions, which belligerents have an equal right to impose. The case when a neutral and a belligerent share cables or land telegraphs would be in itself a weighty subject for international legislation; for the facilities with which the position of the neutral might be turned to account are so obvious as only to increase the jealousy on all sides. By a pre-arranged understanding, cunningly adjusted trade communications, with a sober, business-like sound in their wording, might easily be made to convey important tidings or suggestions of tactics. The possibility of some such manœuvre would, of course, be felt by each belligerent, and would produce perpetual anxiety about the wires which cross its enemy's frontiers from a neutral state beyond, or about the cables which connect its shores with foreign coasts abroad; and that anxiety would create constant suspicion, and in many cases might induce actual attack. The "tone" of an impartial press, the attitude of a neutral Government, would be jealously watched by the belligerent who might not happen to share any overland wires or submarine cable with the non-belligerent State. Indeed the tendency of the present day is to develope this jealousy of neutrals felt by belligerents, as witness the decision of the tribunal at Geneva. The opposite feeling existed in former days, when the increased demand for arms and ammunition caused by the declaration of war was looked upon by merchants of neutral states as a legitimate spur to certain branches of commerce. The increase in the difficulties inevitably following a declaration of war has doubtless contributed to bring about this reaction. For the same reason belligerents are more jealous of neutrals than they used to be. Again, owing to the development of telegraphy, all the nations are more in mutual contact. Each sees more of the game than used to be practicable; each is, in fact, more of a bystander who could help either belligerent if so disposed. We have to take these things into account. Since nations now have this concrete tie of telegraphic machinery—representing in itself so much expenditure and profit, and forming a bond of union for which bygone days offer no precedent whatever—it behoves all the States concerned to devise some means for preserving the property, which no one Power can alone protect. Already we have the opinion of Sir James Anderson that no land telegraphs or cables need be destroyed during war-time, all nations being sufficiently enlightened to "arrange conditions to control or suspend" them, as it may seem advisable, "without resorting to the barbarism of destroying them."

On the other hand, so long as neutrals observed the conditions imposed by the state of war, they would obviously retain the right, not only to continue the ordinary traffic by cable or wire, but also to ally together for mutual protection of property in telegraphy by land or sea, and to send any trade messages, perhaps even into a belligerent country. Again, if a belligerent, in order to cripple her adversary, were to sever the wires or cable of a neutral, that neutral would in its turn have a distinct claim to regard the offending belligerent as having made war upon her, and to act accordingly. Peace must be considered before War, especially in such a case as the present. International telegraphy is a *sine quâ non* of international commerce, and is a necessary means of social intercourse. But there is yet another cause it serves—it is the staunch friend of Science. To cite a prominent instance, Meteorology—a science long kept back for want of copious data obtained simultaneously from distant places—has recently taken a gigantic stride through the sole agency of telegraphy. In fact, we sum up the innumerable boons conferred upon us by the telegraph when we say, that it is the last and most effective addition to the machinery of progress; so much so, that if we might trust the sanguine, we might almost believe the epoch of War to have passed its meridian!

That happy time, however, has not yet been attained; and meanwhile we have other things to attend to which are at least as practical and more immediately urgent. A new kind of international property has sprung into existence, and it demands protection against its own special risks. It is therefore desirable that we should have rules and regulations for the protection of international telegraphy. The case is just one of those for the consideration of the Periodical Congress which the Emperor Napoleon suggested as a means of international legislation. But, in the meantime, Governments and Directors of private telegraphic companies should consider their position while they have yet time; for it would indeed be a matter for grave reproach were they to find themselves unprepared when they realise the emergencies which can be foreseen.

MR. SCUDAMORE AT HULL.

Mr. Scudamore has recently been telling the people of Hull all about the Post-Office, its origin, its growth, its reforms, and latest developments in the shape of the money-order system, the Savings'-Bank system, and the telegraph. He described the marvellous process of its elaboration, from the first invention of the post by Palmer—who, instead of the old mode of sending a letter by a special traveller, stationed men to carry letters on horseback, from one "post" to another—to Sir Rowland Hill's reform. That immortal measure abolished the old charge calculated on distance, and established the uniform penny postage; and was inevitably followed by measures to use the Post-Office machinery in sending money, then in keeping money; and quite lately, by Mr. Scudamore's admirable suggestion, to let the State adopt the newly-invented system of telegraphy.

The effect of Mr. Scudamore's speech forces upon us the consideration that splendid results would be obtained if other active servants of the State would go into society and tell their official story with the same ingenuous distinctness, giving us their experience in practice, their observation of reforms, and the ideas which those tangible data suggest for further improvements. In early days the belief was universal that the State could do everything for the country; then came a reactionary feeling, which looked with bitter jealousy on the "interference" of the State, and indeed denied efficiency to all "place-holders," as it was then the fashion to call the public servants. Our notions on that subject, however, have been materially enlarged since the days even of Mr. Scudamore's great authority, Adam Smith; for while we have practically learnt the enormous gain to be secured from free trade by all classes of Society, and the results of individual enterprise stimulated by private competition, we have also learnt the mode in which those results may be developed in geometrical proportions by combination; provided that the combination be not distorted, corrupted, and stunted by monopoly and class exclusions. Mr. Scudamore shows how the utility and profit of correspondence, banking, and telegraphy have been augmented by the simple fact of bringing in the State machinery and administration. We have thus secured the concentrated organisation which we now see, for communicating with all parts of the country, throughout the day, literally at a moment's notice: we have thus reduced to a minimum the loss through delay, through waste of labour, through uncertainty of information or response; while the very existence of this wonderful machinery has in itself created in the popular mind an utterly new range of ideas as to the occasions on which it may be turned to account. In short, reform has developed reform, and while intercourse has prompted the construction of our postal system, that again has called into existence a totally new system of intercourse; the gigantic State machinery which is co-extensive with Three Kingdoms thus becoming, literally, a domestic servant,—one of the daily tools of the household, the shop, or the office.

ON TELEGRAPHIC BATTERIES.

By JAMES GRAVES,
Engineer to Anglo-American Telegraph Company.

An experience of nearly twenty years in practical telegraphy has enabled me to observe in a variety of places throughout Great Britain the various arrangements of batteries generally used for telegraphic purposes; and so far as I can trace up to the present time very little practical modification is made in the battery-room to meet the very variable conditions under which the lines have to be worked, the general practice being to place a fixed number of cells for a line, and to keep it constant for all weathers; the result is that in fine dry weather the line works splendidly, and in bad weather most wretchedly, if at all.

If anything is done towards modification to meet existing circumstances, it is generally to add more cells in series to force the currents through to their destination, thus increasing the tendency to leakage, and perchance making matters worse.

In looking over some notes made during the manufacture of the Atlantic Cable in the winter of 1864-5, when I had a battery of 400 cells of a peculiar construction and a Thomson's electrometer at my command, I came across two series of experiments with conclusions drawn from them, which I now propose laying before my co-workers, with a view to elicit other opinions, and in the hope that those who have the means at their disposal *now* (which I regret I have not) may confirm their correctness, or throw further light upon the subject.

The battery of 400 cells by every care and attention was kept as free from leakage as possible, and was used for all the tests of the 1865 cable taken daily on behalf of the Atlantic Telegraph Company during its manufacture. Its construction was devised more with a view to constancy than to high electromotive force. Each 100 cells gave exactly the same tension, and, when combined, the corresponding multiple of that tension, showing the perfect equality of the battery throughout.

The following are the two series of tests referred to. The resistances are given in Varley units, but these may be converted into ohms by multiplying by 25.

First Series.

No. of cells.	Full tension.	Closed circuit.		
		Resistance V. U.	Tension in use.	Tension spare.
100	27	25	8	19
200	54	50	16	38
300	81	75	24	57
400	108	100	32	76

Second Series.

Electrometer slightly increased in sensitiveness.

No. of cells.	Full tension.	Closed circuit.		
		Resistance V. U.	Tension in use.	Tension spare.
100	30	250	23	7
200	60	500	46	14
300	90	750	69	21
400	120	1000	92	28

To make the data more complete it would require other experiments to be made; for instance, with a fixed power through a greater number of intermediate resistances between *nil* and infinity, showing how near an approach could be obtained to the full tension in a closed circuit; and also experiments with variable powers through a fixed resistance, noting the effect upon the electrometer on closing the circuit, and thereby showing how much of the full tension is used, and how much is spare.

But incomplete as my own data are (and in that state I submit them), they show that, starting from a given base, any multiple of power, through a corresponding multiple of resistance, gives the same multiple of tension necessary to charge the circuit fully, and a corresponding multiple of spare tension not in use representing the fall from the full tension upon closing the circuit.

It would appear, therefore, that a circuit of 100 V.U. requires a tension of 32 divisions of the electrometer used in these experiments, and the number of cells which gives a tension of 32 is sufficient for this purpose, above which all extra cells are useless.

Of the particular battery used it required 3·7 cells to give 1 division, therefore it would require 118·4 cells to charge the circuit of 100 V.U. to its fullest tension.

Similarly a circuit of 50 V.U. requires a tension of 16 divisions or 59·2 cells to produce a similar result, and a still shorter circuit of 25 V.U. requires a tension of 8 divisions or 29·6 cells, beyond which the addition of any number of cells would be superfluous.

If for the last named circuit, instead of 29·6 cells, we put on 118·4 cells, three-fourths of such a battery would be unnecessary, or, in other words, only one-fourth of its total tension would be called upon to exert itself in such a circuit.

A battery may, in this respect, be compared to a man who can so regulate the degree of exertion necessary, as to lift a hundredweight or an ounce with equal promptitude when called upon to do so, the proportion of strength exerted being in proportion to the weight to be lifted, similarly as the exerted tension in a battery is proportionate to the resistance in the circuit to be overcome. The tension is *nil* in a closed circuit of no resistance (like a man at rest), and rises regularly and gradually through all the intermediate stages of resistance up to its maximum tension, which is only reached when the resistance is infinite, *i.e.*, no circuit, or a disconnection (like a man exerting his utmost force upon a weight which is still immovable).

A different form of battery from that here used might give the necessary tension with a less number of cells, but the principle remains the same.

Reducing the two series of experiments to the standard of the second, we see that—

Cells.	Through V.U.	Exert a tension of.	Out of a full tension of.	Leaving spare.
400	0	*nil*	120	120
400	100	36 nearly	120	84
400	1000	92	120	28
400	Inft.	120	120	*nil*

The degrees of tension in use varying, according to the resistance in the circuit, between *nil* (short circuit) and 120 full tension (loss of continuity).

Having determined the resistance of the circuit to be worked for telegraphic purposes by means of a differential galvanometer, or Wheatstone's Bridge, an electrometer can be used to determine what is the requisite tension for such a circuit, and the proper number of cells can then be ascertained.

For example, reverting to the first series of experiments, the tension required for a circuit of 25 Varley units, or say 50 miles of No. 8 iron wire, is 8 divisions. Supposing wind and wet set in with other ills that wires are heirs to, and that 100 cells are found inadequate for intelligible signalling purposes, we have in these 100 cells a full tension of 27 divisions, and the line requires (owing to the leakage reducing its resistance) rather less than 8 divisions.

It remains, therefore, to point out the great advantage in compensating for this leakage by sending a greater quantity of electricity into the line, whilst still retaining a sufficiency of tension for the resistance of the circuit.

If these 100 cells be now divided into two sets of 50 cells each, joined with their like poles together for quantity, we shall have still 13·5 divisions of tension and double the volume of current entering the line.

By making three sets of 33 cells (throwing off the odd one) joined in like manner the tension will still be 9 divisions, or 1 division (equal to 3·7 cells) in excess of the tension required, whilst the quantity of electricity sent into the line is trebled.

The reduction of the tension from 27 to 9 reduces in the same proportion the tendency of the current to leak out at the faulty places. Whilst the urging force for leakage is thus reduced to one-third, the quantity of electricity circulating is trebled, so that a nine-fold advantage is obtained by using 99 cells in three sets of 33 with their similar poles joined together for "quantity," instead of 100 cells joined in one series for tension, or intensity arrangement.

Another important point is too often overlooked in furnishing batteries for telegraphic lines, *i.e.*, that, irrespective of the length of line to be worked, the size of the battery plates is kept nearly constant. One form very extensively used is Muirhead's chamber battery; the sizes of the plates in these vary (as far as I have been able to discover) from copper 2⅜ × 3¼ inches to 3⅛ × 3⅝ inches, the zinc for each being 1⅞×3¼ inches. These will do admirably for short lines well insulated, but on long lines, with either bad insulation, or good and bad mixed, or subject to bad weather-contacts, from branches of trees, damaged insulators, and other causes, the enormous general leakage runs away with so much of the quantity of electricity supplied, that there remains very little for practical use on its arrival at its destination.

The size of the plates should, in some degree, be regulated by the length of line upon which they are to be used, and much remains yet to be done in this direction. There should be standard sizes for various lengths, such as 50, 100, 200, and 300 miles or upwards; but to use the same size for all lengths of line must be a self-evident mistake, which will doubtless, sooner or later, be rectified by those who have the power to do it.

Valentia, December 9th, 1872.

PROPAGATION OF THE INSTANTANEOUS CURRENT OF THE LEYDEN JAR.

PROF. GUILLEMIN, of Saint Cyr, has recently investigated this subject in a series of experiments, of which we propose here to give some account.

The attention of physicists has of late years been directed to a subject which was long neglected, viz., the variable state of the electric current. Experiments with long telegraph wires have brought into prominence the phenomena produced at commencement of the current,—phenomena which readily escape notice in short conductors. In metallic wires of some metres, or even kilometres length, the variable state is of very short duration, and its existence can only be demonstrated by very complicated experimental arrangements. This remark applies to currents of the pile. But the case is different where a current of the Leyden jar or electric battery is employed. This current is variable from its commencement to its close,

The experiments about to be noticed appear to throw doubt on a law which has been well demonstrated, viz., that the surface of the conductor is without influence on the intensity of a current. This apparent exception, however, is explained (as the Professor points out), when we consider that induction only takes place during the variable state of a current, and that as the current of the battery (Leyden jar) shows the variable state, *par excellence*, the phenomena of induction must become more apparent where it is used. The effects in question are due to the inductive action of the various parts of the conductor on each other.

As to details of method in the experiments,—to measure the intensity of currents Prof. Guillemin employed simply a thin metallic wire (1-10th of a millimetre in diameter), through which the current was passed, and which thus became more or less heated. This may be called the *test wire* (*fil d'essai*). The battery consisted of six large jars, giving a total surface of about 1 square metre. The tension was measured by a calibrated electroscope. The battery was charged from a Rhumkorff coil, giving a spark of 35 centimetres, ten or twelve sparks generally serving for a charge. The induction coil was connected with eight large Bunsen elements.

It is known that the smallest interruption of the conductors will stop the current of a pile. The battery current, on the other hand, was found to be not sensibly hindered by a layer of air ¼ m.m. in thickness. In the experiments showing this, the current between the armatures of the battery could follow two paths,—a metallic wire, E F, and a conductor, E C D F, the latter being interrupted by a layer of air, and also containing at one part a piece of the test wire, 8 centimetres long. (The letters here used have no reference to Figures.) First, before interruption of this conductor, a discharge was passed (from E) sufficient to melt the test wire; the latter having been replaced, the conductor was then interrupted; another discharge was passed, equal to the first; and the test wire was melted, almost as in the first case. When the cut wires were terminated by metallic balls (leaving an air space as before), the test wire was similarly melted. But the effect was much greater when sharp points were substituted for balls, and the air layer might in this case be considerably increased without the fusion being lessened.

From these and some other experiments with lightning conductors, Prof. Guillemin infers that it is not indispensable that the latter apparatus should have a complete metallic continuity, and the thin layers of metallic oxide which generally cover the surface of a conductor may with safety be neglected.

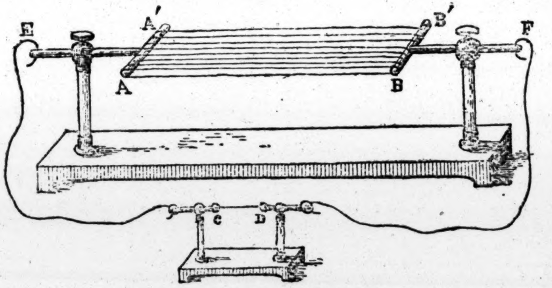
Here, then, is one fundamental difference between the modes of propagation in the current of the pile and in that of the battery.

Next, as to the influence of conductor-surface.

A sheet of tin, 2 metres long and 20 centimetres broad, was placed on insulating supports, and could be metallically connected at either end with the battery. On connection being made, the battery current passed through the plate, and also, by another path afforded it, to the test wire. On a first passage of the current the test wire was heated to a dark red. The plate was next once folded on itself, reducing the surface a half without changing the section; on passage of a similar current the test wire was heated more than before. On further folding the effect was increased, till at length, the surface being sufficiently reduced, the test wire was fused.

Similar results were obtained from other experiments, one of which may here be described. The annexed figure shows the arrangement. Instead of the tin plate, two metallic bars, A A′ and B B′ 15 to 20 m.m. in diameter and 30 c.m. long, were placed parallel as represented, having a metallic wire, ½ or ⅓ m.m. in diameter, wound round several times from one to the other. A test wire 8 c.m. long being placed as at C D, a discharge was passed from E to F, which raised it to a dark red heat. Next, the wires, A B, A′ B′, were brought nearer together, and tied with a silk cord. On passage of the current again, the test wire was fused. (It may be remarked that the wires E C D F ought to have greater resistance than the wire between the bars, that the latter may be traversed by the greater part of the battery discharge). From this experiment it further appears that instantaneous currents going in the same direction retard each other.

Fig. 1.



Another experiment gave additional proof of this. Two metallic bars were placed as in the above figure. Between them were stretched three wires, two joining the ends, and one in the middle, equidistant from both, but capable of being approached to either. They may be named, in order of position, A, B, and C. The distance between the bars was 2 to 3 metres, and the wire interval ½ metre. A and C contained each a piece of test wire, 8 c.m. long. If now a discharge was passed by the conductors joined to the middle of the bars, the test wires were heated equally to a red heat (the discharge being of suitable intensity). Then B was approached, *e.g.*, to C, and an equal current was passed. The test wire in A was completely fused, while that in C was only reddened.

The converse proposition was also found to hold good, viz., that two instantaneous currents going in contrary directions accelerate each other. The conductor, which joined one end of the apparatus just described to the external armature of the battery, passed under the wires at about 1 metre distance from the wire B, and was equidistant from A and C. The wire B being now removed, and a current of certain intensity passed, both test wires were heated equally, but when the lower conductor (just referred to) was approached to one of the remaining wires, the test wire of this was fused, while that of the other remained intact.

The influence of surface, as shown in the first experiments, is now easily explained. A cylindrical conductor, such as a metallic wire, may be considered as made up of an infinite number of infinitely thin wires. If this wire be passed through rollers, so as to change it into a thin plate, the elementary conductors are put further apart, and thus, the retardative action of their currents being decreased, the propagation is more rapid.

A practical application may (the Professor remarks) be deduced from this. A lightning conductor should act better when in the form of a plate than in that of a cylinder. It is necessary, however, to take into account the solidity of the conductors, which might not be sufficient in the form of plates of 1 m.m. thickness. In English ships the lightning conductors frequently consist of plates fixed to the masts, and these have been found to give very good protection.

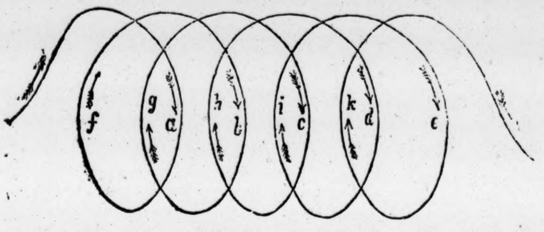
It appears, also, from what has been said, that the configuration of conductors must have a marked influence on the battery current, according as they are straight-lined, or spiral, or arranged in other geometrical forms,

The spiral form was found generally to retard the battery current. This was the case on comparing two equal gutta-percha-covered wires, 12 metres in length, one of which formed a spiral with diameter 3 or 4 c.m. On diminishing the diameter, however, to 1 or 2 c.m., the resistance diminished, and became equal, or even inferior, to that in the other wire (which had a circular direction). Elongating the spiral diminished the resistance.

These phenomena are readily explained. In the neighbouring parts, *a b c d* (Fig. 2), the currents go in the same direction; they go in contrary directions in *a* and *f*, *b* and *g*, &c. The action in the latter is accelerative; in the former, retardative. It is obvious the *latter* influence is apt to predominate when the diameter of the helix is large; the *former* tends to predominate as the diameter is diminished. Hence the different results in spirals of different diameters. When the spiral is elongated the parts in which the current is going in the same direction are drawn apart, while the other parts approach each other,—whence arises a diminution of the resistance.

In any particular case it is a matter of simple calculation what length of spiral will give the minimum resistance.

FIG. 2.



Prof. Guillemin proposes, in further research, to inquire whether phenomena of the class above described may be detected in the current of the pile; also to investigate the influence of the nature of the conductor-metal on the propagation of the instantaneous current.

ON TESTING THE COPPER RESISTANCE OF SUBMARINE CABLES.

By SAMUEL E. PHILLIPS.

We essay to treat this subject from a twofold point of view—(1) as regards the simple *practical* aspect, embracing the essesntial principles involved; and (2) as leading on to the profounder relations which are familiar to the few, and which should be the legitimate study of the many.

It is a pity that most writers affect to occupy the higher platform. Rightly seeing that all electrical questions are embraced in a wider estimate of numerical or mathematical relations, the plainest subjects are obscured to the many by clouds of algebra, and the anxious working student is too often deterred from mastering problems which not only lie in his immediate path, but which really are sufficiently covered by a simple estimate of electrical principles.

We will assume the possession of a few Daniell or Leclanché cells, with an internal resistance of about 4 units each, resistance box, bridge, and suitable galvanometer; and, further, that an ordinary copper resistance test is well understood.

In this case two or three cells would give good differential indications, or with a tolerably good galvanometer one cell would be amply sufficient. The *a* and *b* branches are equal, because the unknown x is assumed to be within the range of the box, 10,000, without any multiplying function; we plug out 1000 on each side in preference to hundreds or tens, because the middle is the best position for the galvanometer.

The full tension or potential is a maximum at Z, and falls equally down the two branches to E, where it is *nil;* therefore we can only get a zero galvanometer (that is, no current passing) when x equals 2445 units, because the tension at the middle or galvanometer junctions must then be equal.

To state thus much according to our second aspect would require an amount of algebra, with honourable mention of Schellen, Kirchhoff, and others, occupying two or more pages of the TELEGRAPHIC JOURNAL.

What, now, are the complications or disturbances found in the practical application of this simple arrangement?

They are threefold as affecting earth-plates, earth-currents, and temperature.

Attention should always be given to the efficiency or otherwise of the earth-plates and their connections. For testing the internal resistance of a battery we have at the same time good conditions for testing earth. It has always been my aim in such cases to have or to provide two independent earths. With a quantity galvanometer, and such a cell as we have referred to, we may get $\frac{\mathrm{E}}{x}=8^\circ$, and then by interposing resistance $\frac{\mathrm{E}}{x+4}=4^\circ$, whence it follows that x or the internal resistance equals 4 units, because a double resistance gives half the current flowing.

Of course x in this case includes the resistance of galvanometer and connections, but as they need not be more than $\frac{1}{100}$ths of a unit, the extra leading wires to box may almost cancel that amount.

If using any ordinary galvanometer, we assume that it is shunted with a short piece of wire, so as to confine the readings within 8° to 12°, where the degrees are sufficiently equal in value.

If the deflections be taken on a shunted mirror galvanometer, the readings may be 300 and 150 divisions of the scale. Here, then, is a wide appreciation for small defects of earth conductivity.

If one cell or short circuit thus gives 300°, and with the same leading wires one pole of the battery be connected to earth, and the other through galvanometer to the other earth; and we then get 300° as mean of two reversed readings, it hence follows that both are perfect.

If, however, that mean should be reduced to 218°, it would then follow that some dryness or defect subsists equal to a resistance of 1·5 units.

We defer for the present the problem of how to localise such a fault, as it is associated with some kindred puzzles of both practical and theoretical interest.

Meanwhile any sharp electrician would naturally well drench the suspected earth, and probably by so doing restore the normal 300°.

I remember detecting a deficient earth at a cable house 700 miles distant; also testing the insulation resistance of a cable 200 miles long, whose nearest end was 500 miles distant, but must here defer these matters to another connection.

2. *Earth Currents.*

The great law of diversity is by no means confined to the infinitude of the stars in the firmament. He would be clever who could provide two equal sized, equally clean, earth plates that would give no current through a sensitive galvanometer; but, in addition to these minor differences of potential, there are others of greater magnitude, due to certain unknown cosmic influences, which will be embraced in the laws of some future *electro*-astronomy.

Algebraic writers are quite up to this requirement, and by treating this difference of potential as a foreign or additional electromotive force in one of the four branches of an electrical bridge, they can eliminate the same by mathematical considerations of the highest value.

We may, however, reverse the usual dictum herein, and say that in this case "there is a royal road to learning," a short cut to all the practical requirements of the case; for by a simple reversal of the current, and halving the total results, we do virtually eliminate the disturbing influence.

This "royal road" is, however, rugged in a sense that no amount of algebra will smooth over, for the earth current is a variable element, and sometimes to such an extent as to make the test quite impossible.

Generally speaking it requires a certain amount of sharp practice and dexterity, and I prefer circular dials, instead of the plugs of an ordinary resistance box; nor should the needle be too delicately astatic. The test should consist of three pairs or reversals, each pair having an interval of time as small as possible.

In periods of magnetic serenity the test will be tolerably uniform, while under magnetic disturbance the tests will be more variable, and the mean of the mean results only approximately correct.

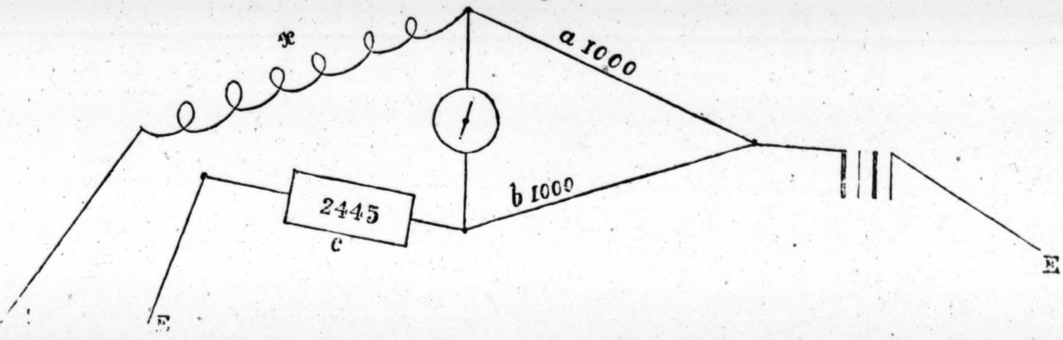


Fig. 3.

The following, from my notes, is an illustration of the former:—

+3855
−3675
+3855 = mean 3765 ÷ say by 500 knots = 7·53 units per knot.
−3675
+3855
−3675

We seldom get this easy regularity, and the figures vary most capriciously, one test often varying more than 100 units during the short time allotted for the operation.

The chief difficulty lies in the peculiar sensitiveness of the bridge, with the varied manipulation of the resistance plugs, and it occurred to me to try and employ the much easier deflection test.

I arranged apparatus accordingly, so that on reversing the current by means of a commutator the same handle moved another commutator, which reversed the direction of the current in the galvanometer. This so far succeeded, that double or treble the number of trials might be made in the same time, or the time allowed diminished one-half; but the range of appreciation was altogether inadequate, as compared with the ordinary balance test. That this test is inherently defiant of pedantic exactitude is further provided for in its dependence on variations of temperature which cannot be strictly estimated or controlled.

As a rule, the temperature of the ocean bottom is doubtless far more equable than the aërial surface; but my experience of the Mediterranean leads me to believe in occasional disturbances, if not regular changes, of greater magnitude than what has appeared in the modern researches of Dr. Carpenter.

If governed by surface temperature, the above test would vary in the Mediterranean and Persian Gulf in round numbers between 60° F. in winter and a mean of about 90° for summer.

As 1° F. adds about 0·21 per cent. to the copper resistance it would be thus—

at 60° 3765 = 7·53 units per knot.
90° 4002 = 8 " "

Before leaving Benghazi (September 1, 1862), I made some trials of temperature at different depths, going about a mile out in a small boat. The observations generally coincided, giving 82·5° at top and 82° at bottom, with 87° in a shallow part near shore, the air being 82° in the shade. At that time I considered the tests to represent a mean bottom temperature for the cable of about 76° E. The large conductor (400 lbs. of copper per knot) was considered 3·65 at 60°, which raised to 76° would equal 3·77 units per knot.

On Sept. 23 I took a careful test of the Tripoli-Benghazi section from Tripoli, the mean observations giving 1915 units, or by calculation 507 knots × 3·77 = 1911·39. The considerable changes of insulation observed upon that line would generally be considered as due to imperfections of the core; but over and above this I thought to trace evidence of variations of temperature due to volcanic and other influences.

During Oct., 1861, Tripoli-Benghazi section fell during five days from 228 to 155 millions per knot. From Dec. 28 to Jan. 25, 1862, the same section rose from 152 to 202, and the Alexandria section was operated on by the same influence, and rose from a lower condition up to 230 by the end of February.

In June, 1862, while Tripoli was gradually falling, Alexandria fell during two weeks from 210 to 119! and the life of the cable seemed in jeopardy.

As to the cause of all this, it may be observed that in the early part of June, when Alexandria fell so quickly, an earthquake took place at Candia, whose influence extended right across the cable to Dernah, where it was felt on the Barbary coast; and before leaving those parts I was informed of some curious facts in support of the views on temperature, which seemed forced upon my attention.

I was informed by a gentleman of great local experience that the sponge divers, before the 15th of August, work in the Gulf of Syrtis (between Tripoli and Benghazi) from 25 to 30 fathoms deep, but from the 15th to the 30th, in order to avoid the approach of cold water, they work in 12 to 15 fathoms, and after that they are compelled to retire to 6 or 7 fathoms. It also appears that there is a strange phenomena of warm and cold currents subject to periodical variation. From October to the middle of November the waters set in the sulphur mines, where it bubbles up and becomes very hot. At one period the current sets out from the gulf eastward by Benghazi to Tokra, and westward by Mursurata to Tripoli; this is about September till February, but after this the currents are both reversed and set into the gulf.

These particulars were only obtained on the point of leaving, were embodied in a report, and it was hoped they might lead to further investigation. Meanwhile, with the curves of testing now before me, and the corresponding variations (as if from temperature) of the two sections referred to, it seems fair to moot that more considerable variations of temperature than are yet acknowledged may be paramount in these effects. It is matter of regret that copper tests for conductivity were not then established as part of the routine of station duties, and what little I did was *con amore*.

In regard to the second aspect, it will be seen that the simplest questions may give rise to endless complications, and in some cases their solution could not well be obtained without recourse to the higher methods of research. The "Bridge" alone is full of such points in relation to the resistance of the galvanometer, the battery resistance, the proportional branches, &c., of which nothing has been written of a popular character.

I cannot here find space to appreciate the bearing of the laws of Kirchhoff, or to entertain the character and fitness of the "Mance method," as recommended by Mr. Winter. I therefore defer these matters, and conclude with the following popular item which has quite recently been pointed out to me.

As few ordinary scale beams are constructed with mathematical correctness, so electrical balances have their special idiosyncrasies; and even the very few whose resistances may be strictly equal at a given temperature, may yet evince a different temperature coefficient and sometimes require slight corrections. If the a and b branches were 10 and 100, then c and x would balance with 20 and 200; but suppose a and b were unknown, or extemporised without admeasurement, and that x (200) were an unknown quantity also? In this case we have three unknown quantities, and all we know is that 20 in box gives zero. It is absurdly simple, but not more simple than true, to say that by doubling c and adding resistance to x another zero might be obtained; 200 would thus have to be added to x, and 200 would be the measure of the unknown x, and the same principle would equally subsist with other proportions. Nay, farther, it would also reveal the unknown fact that a and b were unequal, and that that inequality must be in the ratio of 10 to 100!

PROFESSOR RAOULT, of Grenoble, has proved that nickel employed for twelve hours as a negative electrode in a voltameter, condenses at least 150 times its volume of hydrogen and abandons entirely this gas, when it is taken from the voltameter and plunged in water. M. Raoult has made several experiments on the production of caloric, and has succeeded in demonstrating that the intensity of the heat developed by an electric current is independent of the system of battery by which the current is engendered.

ON THE

ACCIDENTAL CURRENTS DEVELOPED IN A TELEGRAPH LINE ONE END OF WHICH IS INSULATED IN THE AIR.

By COUNT DU MONCEL.

I HAVE aimed at ascertaining the origin of the currents which traverse a metallic wire one end of which communicates with the earth, the other being insulated in air. These currents are relatively strong, and vary not only with the greater or less degree of moisture present in the air, but also with the variation of temperature. With rainfall these currents are negative,—that is to say, directed from the earth towards the end of the wire which is insulated in the air. In dry and fair weather the currents are positive during the day, and negative during the night, while it would appear that there exists a relation between the increase or decrease of the temperature during these periods. When the weather is bad and the sky cloudy the direction of the currents often changes suddenly, sometimes more slowly, according to the appearance of shadows and light from clouds passing by the sun; but always it is evident that the action of the sun has the effect of rendering positive, in relation to the sheet of metal buried in the soil, that portion of the circuit which is affected by the direct sun-light, provided always that the moisture does not interfere. I have proved that these currents cannot be attributed to atmospheric electricity, because, with a perfectly insulated conductor, which is nowhere in communication with the earth, such currents are never produced, and only when a thunderstorm or hailfall is approaching do currents become perceptible by the deviations of the needle of the very sensitive galvanometer used for detecting them.

I explain the production of these currents by considering that the insulated portion of the wire in the air and the buried metal earth-plate form the two electrodes of a terrestrial couple, in which the earth and the supports of the wire—be these telegraph-poles, walls, roofs, &c.—constitute the moist intermediate required to form an element; this couple is then subject to the effects of oxidation, polarisation, and thermo-electricity, that are developed in ordinary couples; and on the predominating effect of either of these causes depends the direction of the current which is observed.

The conditions upon which the production of these currents depends are—(1). That in a hydro-electric element a current can be produced by two strips of one and the same oxidisable metal, if one of these strips is placed in a less moist medium than the other, or, in case the medium is homogeneous, one of the strips happens to be smaller than the other. In the first instance the current is directed across the exterior circuit of the strip placed in the driest portion of the intermediate conductor towards the strip placed in the most moist place, and this is due to the fact that the latter is more readily oxidised than the former. In the second instance the current is directed from the smaller towards the larger strip of metal, because, as the effects of polarisation act more energetically upon the smaller than upon the larger strip of metal, the passing by current is that due to the oxidation of the larger strip of metal. (2). That when a couple is composed of metallic plates placed in precisely the same conditions, and no differential currents are evolved at the ordinary temperature, it is only necessary to heat one or the other of these strips in order to render it electro-negative, thereby causing it to produce a current as if it had a positive pole. If, however, a current is already produced, the action of heat upon the strip of metal causes it to decrease slowly, in case the heated metal was electro-positive,—that is to say, when it plays the part of the oxidised strip of metal, while the deviation of the galvanometer will be increased when the heated strip is electro-negative, that is, yields positive electricity to the circuit. But by continued heating of the metallic strips I have observed that after some time, generally within a quarter of an hour, the direction of the current is reversed, and consequently an action directly opposite to that first made manifest is evinced. I have also observed that moving, wiping, and cleaning the strips of metal give rise to currents, the strip becoming negative in the case of metals which are not readily acted upon by chemical reagents,—as, for instance, with platinum, gold, silver, copper, lead, and tin,—and positive in the case of metals which are readily acted upon,—as with zinc, iron, aluminium. I further prove that heat does not, in this instance, act as it does in thermo-electric currents, but that its action is analogous to the deposition of platinum on the electro-negative plates of Smee's battery, or to that of the deposition of carbon dust upon the graphite plates of Leclanché's, Chutaux's, and other batteries,—that is to say, that it *increases the electro-negative force* of the plates which are thus treated; but *the presence of a resisting and moist medium between the plates or strips of metal is indispensable.* When, therefore, metal filings or graphite powder are substituted for water or for wet sand, in the instances of the preceding experiments, no effect is produced by the application of heat; and this absence of effect is also observed when the metallic filings or graphite powder are moistened with water, because the current excited by the liquid is kept on short circuit, within the interior of the galvanic element, by the metallic conductor, which is interposed so that the current does not pass to the circuit proper. With carbon dust, however, the effect is produced, because it offers an enormously high resistance, equal to that of 148 kilometres of telegraph wire. I have further observed that when, alternately, the two electrodes of a battery which is energetically polarised and those of a battery which is only slightly polarised are heated, that the heating of the electronegative plate or strip of metal (that is to say, that which forms the positive pole) not only causes *the increase of its polarity, but also diminishes the effects of polarisation* therein evolved.

With the aid of these data I have been enabled to explain all the effects which are the result of the accidental currents with which I started my experiments. These currents are, in fine clear weather, positive in the day and negative during the night, because the insulated end of the wire, while becoming heated by the sun, is rendered electronegative in relation to the buried earth-plate or sheet of metal; by inverse reasoning the currents are negative during the night, and also negative when it rains, because then the earth-plate is surrounded by a comparatively drier medium than the wire, which becomes wet.

As regards the inverse actions taking place in this kind of element, these actions are due to the antagonism which exists between the calorific effects, the effects of oxidation, and the effects of polarisation, and which are all developed at the same time. Indeed, let us suppose that one of the two plates of copper constituting one element, placed in wet sand, is heated, so that it becomes negative, and produces a current towards which it plays the part of a positive pole; under this influence the polarisation which might have affected this plate is partly destroyed and the plate is more readily oxidised, and the effect of the oxidation counteracts that of the heat, which, after a certain lapse of time, is not sufficiently strong to overcome the resistance it meets: this causes an inversion in the direction of the current, and this effect continues as long as the plate is heated, that is, depolarised; but when the heating ceases the effects of the polarisation return, and a new inversion tries to bring about the first-observed effects.

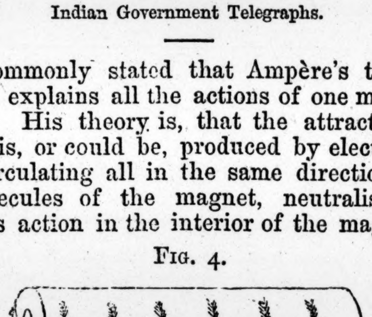
My experiments prove that the currents noticed by Matteucci on telegraph lines placed in the direction from north to south, or from valleys uphill, which currents are in the first case directed from south to north, and in the other from below uphill, are only currents due to calorific action of the same kind as those noticed by me, because the ends of the line which in these two cases are most heated constitute the positive poles of kinds of terrestrial elements.

NOTES ON AMPERE'S THEORY OF MAGNETS.

By W. E. AYRTON,
Indian Government Telegraphs.

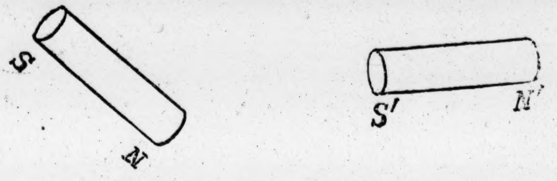
It is commonly stated that Ampère's theory of magnets explains all the actions of one magnet on another. His theory is, that the attraction of a magnet is, or could be, produced by electric currents circulating all in the same direction round the molecules of the magnet, neutralising one another's action in the interior of the magnet, but being equivalent to a series of currents all circulating in one direction round the exterior of the magnet. Consequently, if two magnets were placed with their axes in the same straight line, and with their poles of contrary name opposite one another, the currents would circulate as in Fig. 4, N S, N' S' referring to the commonly called north and south poles. Now as currents flowing in the same direction attract one another, every portion of the magnet N S would attract every portion of the

Fig. 4.



magnet N′ S′, and *the magnets would tend to approach one another, even if the distance between their centres was considerable compared with their lengths.* But it is well known that when the distance between the centres of two magnets is great compared with their lengths (Fig. 5), and the two poles N and

FIG. 5.



S are at about the same distance from S′, their actions on S′ will be equal, but one an attractive and the other a repulsive action. Similarly the actions of N and S on N′ will be equal, the one attractive and the other repulsive. *The whole action will therefore be simply directive,—that is to say, the two magnets will place themselves with their axes in the same straight line, but will* not *tend to approach one another.*

ON THE

ACTION OF POWDERED CARBON HEAPED AROUND THE NEGATIVE ELECTRODES IN CARBON PILES.

By COUNT DU MONCEL.

ABOUT a dozen years ago Dr. Reinch proposed to surround the carbon electrode of his battery with coarsely powdered carbon, in such a manner as to form around it a conducting filter, which would, he believed, increase the power of the pile, especially if care were taken to remove by sifting the fine powder produced by the pounding of the carbon. This arrangement has been since applied in France, by M. Fortin, 1865, by M. Leclanché in 1866, and by M. Chutaux in 1870, in the different piles which they have combined, and they have always obtained excellent results.

We shall have an idea of the advantages that the system of electrodes furnishes by the value of the electromotive force of the bichromate of potash pile, which, with the arrangement of M. Chutaux, is represented by 1·955 (the electromotive force of a Daniell element being 1), while it scarcely attains 1·86 with the ordinary arrangement of simple electrodes. I wish to give a reason for the causes which are able to intervene in this advantageous result, and I have undertaken a series of experiments, of which I am about to give an account, showing that the action of the pounded charcoal surrounding the negative electrode of carbon piles is exactly similar to that which is produced by the platinised platinum in Smee's battery—that is to say, augment the effect of the electro-negative polarity of this body, or its positive polarity with regard to its exterior circuit.

For these experiments I have had recourse to an extremely delicate galvanometer, having 30,000 turns. I have been able to ascertain that retort carbon, under certain conditions, developes an electromotive force which does not exist to the same degree with all carbons, but which varies less for a given carbon, when we do not change its physical conditions. In consequence, I have commenced my experiments by arranging the different carbons that I had at my disposal according to their electromotive power. I pounded two of the most electro-positive and two of the most electro-negative carbons, and mixing them with water I had two mixtures intended to surround the two carbons, serving as electrodes.

I chose those carbons that differed as little as possible in their electric polarity; but before making the experiments I wished to ascertain the relative polarity of the two mixtures, by placing the one in a porous vase and the other around this vase. I found that the mixture made with positive carbons had a positive polarity sufficiently powerful to give with the negative mixture a deflection of 83°. When the two carbons serving as electrodes had been once plunged in water, the direction of the differential current which resulted was 25° to 30° in favour of the carbon, which we will call A. The other carbon, which we will term B, then had with regard to A a negative polarity. This ascertained, I had the carbon B in the water, and I introduced at the centre of the positive mixture occupying the bottom of the porous vase, the positive carbon A, the porous vase having been soaked in water. A considerable deflection, announcing a change of polarity of the two electrodes, was immediately produced, and the deflection in the direction to the left of the galvanometer remained for some moments at 72°, but it slowly descended and shifted to the other side at the end of a quarter of an hour; it then mounted on this right side slowly, but steadily, and at the end of a quarter of an hour had attained 84°. Consequently the mixture had the effect of inverting at the first moment the positive polarity of the electrode A, and later to considerably reinforce this polarity.

After this first experiment I withdrew the electrode A from the mixture, and having washed it carefully, I replaced it in the water, facing the electrode B. I obtained a current stronger than in the first experiment; this current showed that the carbon A had gained in positive polarity, for in place of a deflection of 30°, it furnished one of 51°, which at the end of an hour and a quarter had become 55°. In order to render this experiment complete it was necessary to make a counter-experiment—that is to say, to place in the mixture the carbon B, which was negative, in place of the electrode A. This experiment showed, like the first, the presence of a primary current in the direction of the negative polarity of B, and which became so great at this point as to furnish a deflection of 85° to the right; but as in the preceding experiment the current diminished in energy at the end of an hour and a quarter, there was an inversion of polarity. After an hour and a quarter, the deflection in a positive direction had attained 73°. Under the influence of the mixture the carbon B had its polarity reversed, and this new polarity was so tenacious that in withdrawing the carbon from the mixture and immersing it in water and wiping it, the current was obtained of the same energy as when it was in the mixture, and in the same direction. At the end of twenty minutes this artificial polarity had in its turn disappeared, to give place after an hour and a quarter to a deflection of 49° in a normal direction.

After I had assured myself of the effect of the positive mixture on the two electrodes with which I experimented, I wished to examine the effect of the negative mixture. Consequently I repeated, with the latter, the experiments I had made with the former. The effects were exactly the same, but less intense. The deflection produced by the electrode A under the influence of the mixture, in the place of attaining 84° in a positive direction, as in the first experiment, attained only 80·5° at the end of an hour and a quarter, after having attained 80° in the opposite direction. The positive direction produced by the electrode B under the influence of the same mixture, instead of attaining 73° at the end of an hour and a quarter, had attained only 55·5°, after having marked 70° in the other direction.

Clearly the result of these experiments is, that the mixture of carbon heaped around an electrode of the same substance has the effect of rendering the carbon negative if it is positive, or of increasing the negative polarity if it possess this already; but at the end of a quarter of an hour this effect gives place to another, which is produced in a contrary direction, and the carbon becomes strongly positive if it is negative, or still more positive if it has already that polarity; and by positive polarity we understand that which furnishes to the exterior circuit positive electricity. We can clearly see from this why the piles in which carbon is surrounded by powdered carbon have a superior electromotive force.

According to M. Leclanché, the more advantageous effect of more coarsely powdered carbon tends to show that finely pulverised carbon opposes a much greater resistance to the transmission of the current than coarser powder. The effects of polarisation with the fine powder affects the state of the carbon plate more than with coarse powder. According to M. Leclanché, it is certain that the effects of polarisation were greater at the end of a quarter of an hour than with coarse powder and the ratio of 0·300 to 0·082. It is possible also that some of the advantages which have beeen cited as arising from the carbon mixture consists in its constituting around the carbon plate a larger conducting surface, diminishing at the same time the resistance of the pile and the effects of polarisation.

Proceedings of Societies.

SOCIETY OF TELEGRAPH ENGINEERS.

The annual general meeting of the Society was held on Wednesday, December 11th, at the Institution of Civil Engineers. Dr. Charles William Siemens, F.R.S., occupied the chair.

The chairman announced the death of Sir Donald Macleod, who had been a member of the Society for one day only. The President, on resigning office, addressed the meeting on the history and development of the Society during its short existence of nine months. The first meeting was held on the 28th of February, and the members then counted 110. At the present time the number had increased to 353.

A vote of thanks was offered to Mr. Siemens for his exertions on behalf of the Society.

The discussion on "Lightning and Lightning Conductors" was resumed, when Mr. Erdmann said that in India they used Siemens's lightning discharger and polarised relay, which was easily kept clean. He stated, in reply to Mr. Culley's query, that he did not remember a case where a protection failed.

Mr. W. T. Ansell corroborated the statement.

Mr. Newman said that on the London and North-Western Railway they used twisted wire protectors, which had proved effective.

Sir William Thomson said of the explanations of the phenomena of ball lightning extant, that they were more exciting than exhaustive.

Mr. Varley went at length into another defence of his invention.

Mr. Preece thought that Mr. Cromwell Varley's explanation of ball lightning was more lucid than others, and he should be glad to know of the electrification of clouds as presumed in that explanation.

Sir William Thomson said he could not directly reply to this query, experiments with clouds themselves not having been made yet; but it was plausible for them to be positively or negatively electrified. The surface of the earth and the lower stratum of air were negatively electrified as a rule. In fair weather there was probably more positive electrification in the upper strata. It could indeed not be said how much positive and negative electricity the atmosphere contained; but he had no hesitation in asserting his belief of the quantity being zero—that is, a balance of positive and negative electrification. As a rule, he should say the upper strata were positively electrified. The investigation was one fraught with extreme difficulty, although some experiments could be readily made, such as the collecting of rain and water dripping into an insulated jar. Showers of rain might bring back positive electricity, and there was a discharge of negative electricity from the vegetation. It was generally assumed that clouds were the only condition of electrification, but there were storms without clouds. In some Latin author a thunder storm taking place in clear air was described. The essence of the subject we really knew nothing about at present. It may be supposed that a column of air rose from the lower to the higher strata; there would then arise a disturbance of the electrical equilibrium, and the mere changes in the formation of air would produce electrification. Mr. Varley's experiment was extremely instructive. The electric spark itself might be looked upon as a crack in the air;

Mr. Latimer Clark said that fire-balls were mysterious subjects. Some present might have seen the description of a recent account of a huge revolving fiery mass having made its appearance during a storm at Banbury. According to the description given it was like a ball of fire emitting sparks. In reference to Mr. Preece's allusion to lightning dischargers, he thought that points must be preferable to flat surfaces, as used in Siemens's instrument. He would recommend a flame discharger where the resistance was small, that is measurable in ohms. He believed, with the author of the paper, in the efficiency of a small wire for lightning conductors. The breaking up into several branches was, perhaps, more common than was generally supposed. An ordinary spark could thus be easily divided by the fingers of the hand; in illustration he related the discharge of lightning on his house during a recent storm. The reason why houses were not more often damaged might be sought in the fact of their being protected by the roof-lead and water pipes. Mr. Clark continued, that if it were presumed that a rotating disc, producing the appearance of a ball, had been carried along by the hurricane, the sparks would be plausible.

Mr. Fischer said that probably others would have noticed that in the tropics a general discharge seemed to diverge from one central point, in bundles as Mr. Latimer Clark had put it. There must be then some modification in the theory that the electric discharge took the path of the least resistance.

Mr. W. H. Preece, in his concluding remarks, said that Varley's lightning bridge* had proved a failure as a protector. In reply to Professor Abel, it was no doubt dangerous to use electricity of a high potential for explosive purposes. Mr. Douglas Galton's remarks he hoped would lead to the War Department entrusting the Society with a revision of their arrangements. No one present would go away that night knowing what ball lightning was; there was scope left for further discovery, and he would be glad to see the Society establish themselves into a body of observers, making their returns from time to time.

---

* With regard to the controversy upon Lightning Protectors, Mr. Culley writes as follows in the columns of a weekly contemporary:—

The Chairman said that the distribution of atmospheric electricity was a subject of great scientific importance. Its investigation could, indeed, not be as readily pursued in this as in a hotter country, and he joined Mr. Preece in his desire of members becoming reporting observers wherever they might be. Mr. Clark's description of the Siemens's lightning discharger was not quite correct. What were described as flat surfaces were in reality aggregates of numberless points, the surfaces being placed at right angles one from the other.

The meeting was then adjourned.

The first meeting of the session was held in the rooms of the Institute of Civil Engineers, Great George-street, January 8th. Mr. Scudamore, the newly-elected President, in the chair. The members present included M. C. W. Siemens, the retiring President; Mr. Culley, the Engineer-in-chief of Postal Telegraphs; Major Webber, R.E., Professor Foster, Mr. C. F. Varley, Mr. H. Weaver, Messrs. Priest, Graves, and Shaw, Divisional Engineers, and Mr. R. W. Johnston, Controllor of Special Arrangements, Postal Telegraphs Department.

Mr. Scudamore, in delivering his opening address, after preliminary observations, said: I am not now thinking merely of the benefits which the leading engineers of the Postal Telegraphs Department derive from their contact in this room with the engineers of other services, although I believe those benefits to be great. I am looking beyond them to the great body of persons who are engaged in the practice of telegraphy throughout the country; and who, though they cannot attend your meetings, may read the accounts of your proceedings—may learn therefrom that their occupation has attained to the dignity of a profession, that it has the constant attention of men of the highest scientific attainments, and that, great as the progress of telegraphy has been during the last few years, the ultimate limits to its progress have not as yet been discerned by those best qualified to discern them. The existence and growth of your society cannot fail as years go on to attach the great body of telegraph employés throughout the country to their calling, to make them proud of it, and, by making them proud of it, to make them desirous for improvement, and to awaken in them a spirit of inquiry and a thirst for further knowledge which must inevitably bring about improvements. Now, it is above all things necessary that this spirit of enquiry should be awakened in the great body of telegraph-workers, and that they should be filled with a desire for further knowledge. There are, I suppose, in this country, from ten to fifteen thousand persons who are engaged daily in the practice of telegraphy. Most of them are young and have before them all that period of life in which the mental powers are most active. In point of general intelligence and education they are at least on a par with those members of their social grade who are engaged in other services. They have been taught to use—and they do use daily—instruments in the construction of which the greatest possible amount of mechanical ingenuity has been exerted—instruments which might well be expected to arouse curiosity and stimulate thought, and to cause those in whose care they are to ask themselves continually "How is it that these machines produce the results which we see proceeding from them during every hour of our lives?" and never to rest until they found an answer to the question. And when they had got an answer to this question, we may be quite sure that other questions would follow; that the workers would want to know whether the same results could not be produced in other ways, or more rapidly, or more simply, or more cheaply, and, that, from the once-awakened curiosity, improvement and development would inevitably proceed. Now, I am afraid that in the minds of the great majority of the telegraph-workers throughout the country, this laudable, this desirable curiosity has not as yet been awakened. There are exceptions—highly honourable exceptions, some of which I shall hope to bring under your notice in the forthcoming year—but I am afraid that the great majority of telegraph-workers regard their instruments merely as rather troublesome pieces of machinery, which are very easily deranged and very difficult of adjustment, and that their first—and indeed, their only—idea when one of these pieces of machinery gets out of order, is that somebody had better come from somewhere to put it right again. Don't suppose that in saying this I am finding fault. I am merely describing what I believe to be a natural, not a blameable, state of things. There are in the world many millions of watches; but how few of the owners of those watches have ever even thought about the mechanism of the watch which they carry about with them all their lives through! This is a circumstance which concerns none but the owners of the watches, but the indifference—and I must beg you to understand that I do not use the word as a term of censure—the indifference of the telegraph workers to their instruments concerns the nation; and if your society can, even in part, substitute interest for indifference, it will render a service to the country. Probably some one will ask me whether the employers of telegraph workers might not give the required stimulus in the shape of promotion. To this obvious question the answer is equally obvious. Promotion must be given to those who do their allotted work well and faithfully, and it is difficult to reward by promotion those who do more than they undertook to do without taking something from those who have done all they undertook to do, but no more. Probably, too, some one will ask why the employers of telegraph workers should not make some systematic effort to instruct the whole of the staff in all that is desirable for the staff to know. I think the answer to this question is equally obvious. It would be useless to try and teach the whole of the staff at once all that is desirable for them to know. All have not, and cannot be expected to have, the capacity for learning all that it is desirable they should know; and of those who have the capacity, only those would learn readily in whom there is at present an existing, though perhaps a dormant, inclination to learn. What we want to do is to arouse and quicken this dormant inclination; and I firmly believe that your society will render us most powerful and valuable assistance. It is my hope and belief that, year by year, a continually increasing number of leading officers in the Postal Telegraph Department will enter the ranks of your society, that year by year the attrition between their minds and the minds of other telegraph engineers who may be as-

---

"I feel it due to Mr. S. A. Varley to say that in supplying his carbon "lightning bridge" to the Post-office he proposed to connect it to the earth at every station. For reasons of my own not connected with science, but simply with discipline, and therefore of no interest whatever to your readers, I preferred not to connect to earth at intermediate stations. The impression produced during the recent discussion at the Society of Telegraph Engineers was that this arrangement is a part of Mr. Varley's system. This impression I wish to remove, for looked at from the scientific point of view alone, the arrangement adopted is clearly not as efficient as that proposed by Mr. S. A. Varley.—R. S. Culley, Telegraph-street, December 21st, 1872."

sembled in this room will give rise to fresh developments of their science, and that year by year their example and their precepts will quicken in an increasing number of their subordinates a desire not only to do their work, but to understand, and improve, and simplify the means by which it is done. I have the best possible reasons for thinking that this will be so. I find my reasons in the circumstances which have occurred during the last three years. I find them in the efforts which the engineering officers of the Post Office have made during the last three years to keep pace with the requirements of the public. I say advisedly that they have made those efforts because they have found themselves to be the servants of the public, and that they have effected improvements in the system to which they would hardly have given attention if they had not become the servants of the public. In this country (Mr. Scudamore went on to say) the telegraph service had had to work under the sharp spur of public necessity. The Post Office had become popular because it was efficient, and efficient because it was popular; and with the cheapening of telegraphy he ventured to think that the popularity of that branch had become greater also, and that higher efficiency would still be required. The pressure would become heavier and heavier every year, and would ever continue to afford a stimulus to promote and encourage the efforts of a society like that in achieving further triumphs of science.

A cordial vote of thanks was given to Mr. Scudamore for his address.

The following papers were read:—"*A Direct Method of Determining Battery Resistances*," by Dr. WERNER SIEMENS, of Berlin. "*On Lightning and Lightning-Conductors*," by Mr. GRAVES, of Valentia. Stress was laid on the circumstance that popularly the ascent of lightning from the earth, as well as descent from the clouds, is not sufficiently considered in the construction of protective apparatus. In the course of discussion a gentleman present gave, as the result of his own experiments, the interesting practical conclusion that, for the purpose of attracting lightning from a passing cloud, a gas jet, flaming from the mouth of a tall pipe or conductor, proves far more effectual than a simple pointed rod. Two papers were also read by Sir William Thomson.

ROYAL SOCIETY OF SCIENCES, GOTTINGEN.

August 3. M. W. Weber communicated a paper by M. RIECKE, "*On the Law of Electro-Organic Reciprocal Actions Proposed by Helmholtz.*"

September 25. M. KOHLRAUSCH read a paper "*On the Electromotive Power of Very Thin Layers of Gas upon Metal Plates.*"

November 13. M. Riecke communicated a paper upon the "*Magnetisation of a Sphere of Soft Iron.*"

IMPERIAL ACADEMY OF SCIENCES, VIENNA.

October 10, 1872. Prof. L. Boltzman communicated a memoir "*On the Experimental Investigation of the Behaviour of Non-conducting Bodies under the Influence of Electrical Forces.*"

A NEW process of "tinning" by electricity has been brought into successful operation in Birmingham. By this process any metal, it appears, but zinc, can be coated with a very white metal of great brilliancy, of which tin forms the principal elements. The precipitation is effected as in the electrotype process. It is the intention of the company to tin in this way plates of iron and steel, and to use it as the layer upon which silver may be deposited, so that in ornamental articles, if the silver wear off, a pure white surface shall be still presented to the eye.

Notices of Books.

Reprint of Papers on Electro-Statics and Magnetism. By Sir William THOMSON, D.C.L., LL.D., F.R.S., F.R.S.E., Fellow of St. Peter's College, Cambridge, and Professor of Natural Philosophy in the University of Glasgow. London: Macmillan and Co., 1872.

THAT men of science should rest upon well-earned laurels would not be a startling fact, even if it were true; and that the fact has no existence in truth is a matter clear to the most ordinary observer. There are few of our scientists that commenced work, say thirty years ago, who are not now in their laboratories, still cross-questioning nature as closely as when their first papers were presented to the world. Thirty years' work has brought fame which could not be extended, yet the old pursuits are followed as assiduously as ever. Evidently our great philosophers work not for fame, but indeed as lovers of truth.

There is some slight feeling of pain arising from the perusal of a work such as that before us, when we think how much labour is here unappreciated—unseen and unrecorded labour—the failures before success. Very rarely does a scientific man live to gather fruit from the tree of which he has sown the seed, and never to gather the result of all of his labours. It would be incompatible with the principle of progress that he should do so, for the development of a scientific fact is multiple, evolving many fresh facts which in their turn are subject to the same law. This or some similar train of thought must occur to the student of Professor Thomson's latest work. Three-fourths of the volume consist of reprinted articles on Electro-statics and mathematically allied subjects which have appeared at different times during the last thirty years from the pen of Sir William Thomson in the *Cambridge Mathematical Journal*, *Journal de Mathematiques*, the *Philosophical Magazine*, *Nichol's Cyclopædia*, the *Reports of the British Association*, and the Proceedings of the various societies. The remainder has been written to fill up roughly gaps in the collection. The articles are forty-two in number, aud include as nearly as may be all that Sir William Thomson has hitherto written on eletro-statics and magnetism. There has been excluded any paper in which either thermo-dynamics or the kinetics of electricity is prominent. Useful correspondence between Sir Wm. Thomson and other eminent scientific men adds much to the interest of the volume. There are also many gems to be found among the shorter communications to the various societies; the following extracts from a "New Proof of Contact Electricity," is an instance. "About two years ago I wrote to you (the President of the Literary and Philosophical Society of Manchester) that a metal bar, insulated so as to be movable about an axis perpendicular to the plane of a metal ring, made up half of copper and half of zinc, the two halves being soldered together, turns from the zinc towards the copper when vitreously electrified, and from the copper towards the zinc when resinously electrified.

"If the copper half and the zinc half of the ring are insulated from one another, and if they are connected by means of wires with two pieces of one metal maintained at any stated difference of potential by proper apparatus for dividing the electro-motive force of the two plates of a Daniell's element into 100 parts, from 60 to 70 of those parts are required to reduce the zinc half ring and the copper half ring to such a state that the movable bar remains at rest whether it is electrified vitreously or resinously.

"If the copper half ring is oxidised by heat, the amount of electro-motive force then required to neutralise the two halves is much increased. If, after oxidising the copper one day by heat, I leave the apparatus till the next day, the effect is generally diminished, though something of it still remains. After again heating the copper by laying it for some time on a red-hot iron heater and allowing it to cool, I found the effect almost exactly 100 parts. I have no doubt that by making the coat of oxide very complete and thick enough, and by cleaning the zinc perfectly I shall be able to get considerably above the electro-motive force of a single Daniell's element.

"For nearly two years I have felt quite sure that the proper explanation of voltaic action in the common voltaic arrangement is very near Volta's, which fell into discredit because Volta or his followers neglected the principle of conservation of force. I now think it quite certain that two metals dipped in one electrolytic liquid will (when polarisation is done away with) reduce two dry pieces of the same metals, when connected each to each by metallic arcs, to the same potential."

To the electrician the book is indispensable; and it will be found in a handy form, with a very complete index.

Students' Column.

Laws of the Duration of the Relative Diffusion of the Electric Current.

FOLLOWING M. Gaugain's investigation* of these laws we learn that when the loss by the air may be neglected, the laws which have been stated in previous numbers of this Journal, except the first, can be applied to the duration of relative diffusion. In this case the *duration of the diffusion becomes independent of the tension of the source;* and to comprehend this easily, let us suppose that at the end of a time, t, a given section, m, of the conductor has attained c under the influence of a source of tension, E. The conductor remaining the same, we increase the tension to nE. If the proposition be accepted that the tension of a given section of the conductor is always proportional to the tension of the source, the section m at the end of the time t should necessarily acquire a tension, $n\,e$, under the influence of the tension nE; consequently the time necessary in order that the tension of a given section of the conductor should become a given fraction of the tension of the source is completely independent of the tension of that source.

When the loss by air cannot be neglected, and the current is subject to regular derivations, *the duration of the relative diffusion varies less rapidly than is indicated by the general propositions which have been already established;* it follows an inverse law to that applicable to absolute diffusion when the loss by the air or by derived circuits cannot be neglected.

If we seek to embody these different laws in one formula, and consider *the duration of the variable state as a particular case of the duration of relative diffusion,* we should select the formula proposed by M. Gaugain, viz.:—

$$\mathrm{T}=\frac{qcl^2}{k\omega},$$

where T is the duration of diffusion, k the specific conductivity, l the length of the conductor, ω the area of the section, c the coefficient of charge, and q a constant coefficient depending upon the units adopted and the values given to c, l, k, and ω.

The demonstration of the different laws is very simple. We will examine the value of the duration T when an electric source of constant tension is put into connection with two conductors, l and l', of the same dimensions of different conductivity; and, in order to simplify our reasoning, we will suppose the conductivity of l' to be twice that of l. Let also m_1, m'_1, m''_1, designate three successive sections taken in the conductor l, and m_2, m'_2, m''_2, three other sections occupying respectively the same positions in the conductor l'. If we consider that at the end of a certain time, t, *the distribution should be* the *same in the two conductors, it is necessary to admit that the quantity of electricity which the section m'_2 should receive from the section m_2 in the element of time Δt,* should be double the quantity of electricity passing the section m_1 to the section m'^1; in the same manner the quantity of electricity given up by the section m''_2 should be twice greater than the quantity given up by the section m'_1 to the section m''_1; therefore increment of the tension of the section m'_2 in the element of time Δt should be twice greater than the increment of tension of the section m'_1. In other terms, the section m'_1 only possesses at the end of two seconds the same tension that the section m'_2 attains at the end of a single second.

As the preceding reasoning can be applied to all the sections of two conductors and at any successive period, it results that the distribution of the tension established at the end of a time $t+2\theta$, in the conductor l, should be exactly the same as those which appertain to the conductor l' at the end of the time $t+\theta$; θ being any limited interval of time. It can be shown in the same manner that the distribution of tension in the conductor l at the end of the time $t-2\theta$ is still equal to that which appertains to the conductor l' at the end of time $t-\theta$; then, if the tension is *nil* at the end of the time $t-2\theta$ for the conductor l, it is equally *nil* for the conductor l' at the end of the time $t-\theta$; but in this case θ and 2θ represent precisely the interval of time termed, by M. Gaugain, the duration of absolute diffusion. Therefore it may be stated that *the duration of absolute diffusion is in inverse ratio to the conductibility.*

It is evidently the same with the duration of relative diffusion; for the tension limits of the points which occupy the same position in the conductors A and B, being equal to each other, it is clear that if the tensions of the same points have at any given period the same absolute value, they have also the same relative value.

The law relative to the section can be demonstrated by the same method, it being but necessary to substitute for the word *section* the word *conductivity* in the preceding reasoning. It is the same with the law relative to the coefficient of charge.

The law of the lengths is also derived from the same theory. Let us suppose that the two conductors, l and l' differ only in their length, which should be for l' double that for l; we can easily discern that in order that the tensions of the different sections m_1, m'_1, m''_1, m_2, m'_2, m''_2 should become equal two and two, it is necessary that the times for the cylindrical elements, m_2, m'_2, m''_2, should be twice as long as those of the other sections. Accordingly, the electric currents transmitted to traverse the first in the element of time Δt, should be twice less than those which are transmitted in the same interval of time from m_1 to m'_1, and from m'_1 to m''_1. On the other hand, the surface of the cylindrical elements, m_2, m'_2, m''_2, being twice greater, it is necessary, in order to obtain an equal increment of tension, to communicate to the first element an absolute quantity of electricity double that which is communicated to the second. The increment of tension, which is produced in m'_2 in the element of time Δt, is then four times less than that produced in m'_1. Thus, four seconds' flow are necessary in order that the section m'_2 may attain the tension of the section m'_1, acquired during a single second. The same reasoning may be extended to all periods and to all sections; and we may conclude that the duration of the diffusion corresponding to the conductor l' is four times greater than that which corresponds to the conductor l, and more generally, that the duration of diffusion is proportional to the square of the length, the diffusion being considered absolute or relative.

---

* "Exposé des Applications de l'Electricite."

The law relative to the tension of the source can be deduced from the same reasoning. Let us suppose that the sections, m, m', m'', have, at the end of the time t, the respective tensions u, u', u'' proportional to the tension of the source a: the current exchanged in the instant Δt between the sections m and m' should be proportional to a, since this is proportional to $u-u'$; it will be the same for the current exchanged between the two sections u' and u''. Thus, the increment $\Delta u'$ of the tension of the section m' will be proportional to a, since this increment is but the difference of the quantities of electricity received and transmitted. Then, finally, the tension $u'+\Delta u'$ possessed by the section m' at the end of the time $t+\Delta t$ should remain proportional to the tension of the source a. The same reasoning can be applied to all sections and all successive periods; and we perceive that if the tensions corresponding to various points of the conductor are, for a given period, proportional to the tension of the source, this proportionality should exist for all subsequent periods. It may also be shown that this law exists for all preceding periods. Now we know that under permanent conditions the tension of any determined point is proportional to the tension of the source; it is the same under variable conditions. But it is easy to see that this proportionality makes it impossible not to admit that the relative diffusion is independent of the tension of the source; for if we designate by T the tension-limit which appertains to a determined point, P, of a conductor, we shall have $T=ka$; a representing the tension of the source, k the quantity which renders a equal to T, and which is independent of a; but after the proportionality has been established, if we represent by t the tension obtained at the point P at the end of a given time, θ, we shall have $t=k'a$, and from these two equations we perceive that—

$$\frac{t}{T}=\frac{k'}{k}, \text{ or, } \frac{t}{T}$$

represents exactly the duration of the relative period, and we see that the quantity a disappears from the member of the equation which gives its value. The duration of the relative period is thus independent of the tension of the source.*

It remains to explain the causes which modify the preceding laws in the case where the circuit is subject to regular derivation. If we reflect that the duration of absolute diffusion bears a relation to a given tension; and if we consider, on the other hand, that all causes which have the effect of enfeebling the tension of the circuit have also the result of prolonging the duration of diffusion with a given electric force to furnish this given tension, we can easily comprehend that derivations which enfeebled the intensity of the current in circuit, and there increasing with the length of the circuit, should have the effect of prolonging the duration of absolute diffusion, and to affect the laws of proportionality which have been established for this duration. An extremely curious, and in a practical point of view important, consequence results from the introduction into the formula of the coefficient of charge c, it is that we augment considerably the rapidity of transmission in augmenting the diameter of the conducting wires.

In measuring the coefficients of charge for several samples of wires of the diameter most used in telegraphy, M. Gaugain has arrived at the following results:—

Diameter of wires.
1 . 2 . 3 . 4 . 5

Coefficients of charge.
100 . 113 . 125 . 133 . 141;

and we perceive that the coefficients increase less quickly than the diameters. While the durations of diffusion are proportional to the following numbers:—

Diameter of wire.
1 ; 2 ; 3 ; 4 ; 5.

Duration of diffusion.
100 ; 28·2 ; 13·9 ; 8·3 ; 5·6

We shall in the next number conclude these papers on diffusion, with the application of the laws to telegraphic lines.

Laws of Magnetism.

1. A magnetic field is any space in the neighbourhood or under the influence of a magnet.
2. The unit pole is that which at a unit distance from a similar pole is repelled with unit force.
3. The intensity of a magnetic field at any point is equal to the force which the unit pole would experience at that point.
4. The direction of the force in the field is the direction in which any pole is urged by the magnetism of the field; this is the direction which a short, balanced, freely suspended magnet would assume.
5. An uniform magnetic field is one in which the intensity is equal throughout, and hence the lines of force parallel.
6. Opposite poles attract each other; similar poles repel each other.
7. The forces directed from any magnetic point upon equal masses are reciprocally proportional to the square of the distance.
8. When two magnets are very small, and the distance between them very great in proportion to their length, the magnetic action between them is reciprocally proportional to the cube of their distance.
9. The force directed from any magnetic point upon any other mass upon which it acts is reciprocally proportional to the square of the distance. The total action between them both is, however, reciprocally proportional to the third power of the distance when the latter is great.

Electrical Science in Foreign Journals.

(We intend to devote this column to a list of Electrical Memoirs published in Foreign Journals during the month. Those of importance will be either translated in full or given in abstract.)

Comptes Rendus Hebdomadaires des Séances de l'Academie des Sciences, No. 23, December 2, 1872.

On the Magneto-Electric Machine of M. Gramme, applied to Electrotyping and the Production of the Electric Light.—By M. Gramme.—"In July, 1871, I had the honour of submitting to the Academy a first specimen of my magneto-electric machine. I propose, in this second note, to present the practical solutions of electrotyping and for the production of the electric light. For the production of continuous currents I made a circular electro-magnet to turn before the magnetic poles of a magnet, and I obtained the currents in a plane perpendicular to the poles. My movable electro-magnet is composed of a crown of soft iron, not presenting any projection, on which is wound a continuous metallic wire. This metallic wire is divided into a series of little bobbins, which are connected with plates of the same metal. Each bobbin communicates with one of these plates, and they are separated from each other by a simple layer of silk. It is essential that the union of the conductors forms a compact cylinder, and that the insulator should be very thin: without this the machine would not give powerful sparks, and would only produce insignificant currents. The possibility of establishing a number of poles is the most striking point of my invention. It is this which permits the production with a single machine of a series of distinct currents, and of exhibiting the electric light. In order to appreciate exactly the effects obtained by a movable electro-magnet moving before a magnet of known power, I constructed my specimens with two poles only.

---

* "Annales Telegraphiques," iv., 13.

"*Machine for Electrotyping.*—The electrotype machine, which for four months has been in action in the works of M. Christofle, of Paris, is composed of a beam bearing two movable electro-magnets, and of two others horizontal to the consequent poles. It has been calculated that to produce a deposit of 600 grms. of silver a speed of 300 turns per minute is requisite. The wire wound on the electro-magnets weighs 135 kilos., and that of the movable electro-magnets 40 kilos. The force necessary for the normal motion is about 1 horse-power. The tension of the current produced is equal to that of two ordinary Bunsen elements; the quantity corresponding to thirty-two elements. The scrubbers or collectors of the currents are constructed upon a new system; they are composed of a large number of copper wires held together by a band, which gives the form of a brush or a flat broom. This accessory of the machine is an invention applicable to all magneto-electric or electro-magnetic machines. It gives a very soft contact, and prevents break in the continuity of the current from vibration, and the giving off of destructive extra-current sparks. At the rate of 275 turns, the machine has deposited 525 grms. of silver in an hour; at 300 turns, 605 grms.; and at 325 turns, 675 grms."

M. Gramme here gives a table showing the effects produced by his and by Wilde's machine in depositing copper on electrodes of various sizes. The experiments were made by M. Christofle, and the results are 8 to 1 in favour of M. Gramme's machine.

"*Light Machine.*—The problem for the production of the electric light is, as we know, very different; the tension of the electricity ought to be much more considerable and the quantity much less than for chemical decompositions. In the machine with which I experimented, the tension attained 105 ordinary Bunsen elements, and the quantity was found to be reduced to five elements. The arrangement of this machine is vertical; its height is 1·25 m.m., its base measures 0·80 m.m. by 0·80 m.m.; its weight is about 1 ton. As the tension cannot be obtained by the length of wire wound on the electro-magnets, I have, to economise space, established three fixed electro-magnets and three bobbins, or movable electro-magnets, with consequent poles. One of the bobbins developes the magnetism in the fixed electro-magnets, and the two others furnish the current which produces the light. The primary magnetisation was made by the assistance of a battery. The wire wound on the fixed electro-magnets weighs 250 kilogrammes; that of the three bobbins 75 kilogrammes. The spindle of the machine making 300 turns per minute, with a force of about 4 horse-power, I have obtained a light equal to that of 900 Carcel lamps,—that is to say, an artificial light more intense than any yet produced. The calorific effects corresponding to the same speed of 300 circuits presents great interest. I can heat to redness a copper wire, 12 metres in length and $\frac{7}{10}$ths of a millimetre in diameter, and an iron wire $\frac{13}{10}$ in diameter and 5 metres long. I fused an iron wire $\frac{13}{10}$ by 2·50 m. long."

No. 24. December 9, 1872.

On Magnetic Distribution.—By M. Jamin.—"M. Trève has previously communicated a paper on '*Magnetism*,' in which he announces, amongst other things, that the poles of a magnet are displaced and removed from its extremities when a soft iron armature is fitted to them. He believes that he demonstrates this by placing opposite the magnet a magnetic needle, and showing that its direction changes after contact is made with the armature, whilst admitting that the pole is thus prolonged in the direction of this needle.

"First, it is necessary to define the word 'pole;' it must also be remarked that in this experiment the magnetic needle is opposed to the attraction of the steel which forms the magnet, of the iron which constitutes the keeper, and also to the attraction or to the repulsion of the free magnetism of the magnet. Its direction follows the result of these very complicated actions, which depend on the distance, the weight, and the shape of the armature, and above all upon the distribution of the magnetism in the magnet itself and its keeper. It is therefore apparent that the direction accruing from so many diverse causes cannot lead to any definite conclusion.

"That which ought to be first settled in this question is the distribution of magnetism in the magnet, both before and after the application of the keeper. This is a matter which has occupied my attention for a very considerable time, and I take the present opportunity of saying a few words thereupon.

"I studied this distribution by two processes, the one of which corroborates the other. The first consists in placing upon the extremity of a magnet a small soft iron electro-magnet enveloped by a copper wire communicating with a galvanometer. An induction current is thus produced, causing a deflection of the galvanometer, and the measure of the arc of impulsion enables the intensity of magnetism at different points touched to be very readily measured.

"The second process is very analogous to that of Coulomb: it consists in placing upon the spot of which it is desired to study the magnetic character a small sphere of soft iron, attached to which is a device by which it can be dragged away from contact. The tension, at the moment it is detached, is proportionate to the amount of magnetism at each point of contact.

"If the magnet has its extremities free, we can recognise that the free magnetism increases progressively in intensity from the neutral line to the extremities. The curve of intensity is very nearly that which Coulomb has indicated.

"When the keeper is applied all is changed; two poles appear at the two extremities of the keeper, the free magnetism in part disappears. Receding from the two extremities it reaches a maximum, again decreasing towards the neutral line. The conditions of these modifications are very complicated, and require searching investigation. They contribute to produce the changes in direction of the needle examined by M. Trève; but these changes cannot in any case disclose the magnetic distribution within the bar, and it is not desirable to assume that they indicate any change in the position of the poles of the magnet."

On the Thermic Effects of Magnetisation.—By M. J. Moutier.—A note upon M. Cazin's paper given in our last.

On the Accidental Currents in Telegraph Lines.—Note by M. le Compte du Moncel.—The entire paper is given elsewhere.

On Electro-Magnetism.—By M. Trèves.—Given elsewhere.

No. 25. December 16, 1872.

On the Distribution of Magnetism.—By M. Jamin.

Note on Magnetism.—By Capt. Trèves.—An extension of the former note read to the Academy.

On a New Method of Producing Ozone.—A second note by M. A. Boillot.

Journal Telegraphique, No. 12, Vol. 2.

Telegraphic Legislation.—Italy.

Terrestrial Magnetism.

Metal Poles.—An admirable article upon the forms of metallic posts in use in telegraphy.

Les Mondes, No. 15, December 12, 1872.

On the Accidental Currents Developed in a Telegraph Line one End of which is Insulated.—By Count du Moncel.—We give this paper elsewhere.

Note on Electro-Magnetism.—By Capt. A. Trèves.—" We know that needles can be magnetised by the discharge from a Leyden jar passed through a wire which envelopes them, and that it follows that static electricity possesses the same properties as dynamic currents. This property is applicable by means of a Rhumkorff induction coil, by passing the currents through a solenoid of glass containing a rarefied gas. I have introduced between the turns of the solenoid some steel bars which a single discharge sufficed to transform into magnets." Capt. Trèves then proceeds to consider the relation of this experimental fact to Ampère's theory of the earth's magnetism.

No. 17. December 26, 1872.

Thermo-Electricity.—(Notes).—M. A. d'Obermayer has executed a series of thermo-electric experiments on the following combinations:—Iron-tin, iron-lead, iron-selenide of lead, iron-zinc, and iron-bismuth, taking as a starting-point the temperature of the fusing-point of the most easily fusible substance. The results show that the electromotive force of these elements have precisely the same value during fusion as at the moment of solidification.

Obituary.

We regret to announce the death of two eminent men connected with electrical science. J. W. Macquorn Rankine, LL.D., F.R.S., Professor of Civil Engineering and Mechanics in Glasgow University, died on Christmas-eve. Professor Rankine was born at Edinburgh in 1820, and was educated in the university of that city. His pupilage was passed under the care of his father and of Sir John M'Neill. In 1852 he received from the Royal Society of Edinburgh the Keith medal for his researches on the mechanical action of heat. In 1855 he was appointed to the professorial chair of Engineering and Mechanics in the University of Glasgow. Since that time his pen has been most constant in its action, and his death will be lamented by every engineer and engineering-student who appreciates the due application of mathematics to physical or mechanical science. Professor Rankine was also known in his social circle as the composer of a number of songs, two of which, one upon the old stage coach and the other "The Engine-driver's Address to his Engine," appeared in *Blackwood*, and are well known.

The second death we have to lament is that of Archibald Smith, LL.D., F.R.S., of Jordan Hall, Lanarkshire. Dr. Smith was born in 1814, studied at Glasgow and Cambridge, passing in 1836 as Senior Wrangler and first Smith's Prizeman; the Second Wrangler was Bishop Colenso. In 1862 Dr. Smith published his celebrated "Admiralty Manual for the Deviation of the Compass." At different times he received from the Royal Society one of its Royal Medals, from the Emperor of Russia a compass set with diamonds, and a Government gift of £2000.

Mr. Eden, indoor engineer in the Edinburgh Telegraph-office, has invented a system by which, with the existing instruments, it has been found practicable to send messages from both ends of a single wire simultaneously. The invention has been tested between Edinburgh and Glasgow, and it has been found that one wire is capable of doing double work.

Correspondence.

CABLE SIGNALLING.

To the Editor of the Telegraphic Journal.

Sir,—Referring to the notice, in your last number, of my paper on " Cable Signalling," I beg to inform you that Mr. Varley has since told me that he has, on several occasions, tried the induction-coil at the receiving end of a cable, but with results less satisfactory than those obtained with the condenser.—I am, &c.,

G. K. Winter.

2, Wilton Villas, Shepherd's Bush,
January 8, 1873.

FRENCH ATLANTIC AND GREAT WESTERN CABLES.

To the Editor of the Telegraphic Journal.

Sir,—I have to call your attention to an error in your Journal of the 15th inst., appearing at page 35, in which it is stated—

" The manufacture of the duplicate French Atlantic and Great Western Cables is steadily progressing at the Telegraph Construction and Maintenance Works at Greenwich."

The statement with reference to the French Atlantic Cable may be correct, but as the contract for the manufacture and laying of the Great Western Telegraph Cable has been given to this Company, I must ask you in your next issue to correct the mis-statement.

In doing so, as it may perhaps interest your readers, you may be good enough to state that 2000 miles of the said cable has already been manufactured, passed, and approved by the engineers of the Company, Sir William Thomson and Professor Fleeming Jenkin.

A ship specially constructed for the laying of cables is being built for this Company, and arrangements have been made for the laying of the two sections; the first from England to Bermuda, and the second from Bermuda to New York, in the month of June next.—I am, &c.,

H. Winfield Crace,
Secretary Hooper's Telegraph Works, Limited.

31, Lombard Street, London, E.C.,
December 30, 1872.

POSTAL TELEGRAMS.

To the Editor of the Telegraphic Journal.

Sir,—I have noticed some correspondence in the *Times* within the last few days respecting the present mode of receiving and delivering telegrams.

It is certainly very desirable that receipts should be given for telegrams. If you send a telegram to a telegraph station, what proof have you that the person entrusted with it actually hands it to the department? There is no proof; and further, would not the mere fact of giving receipts be a protection against embezzlement by the clerk? It is true, to prevent this, that a certain number of stamps are issued to each station; these stamps are issued to be affixed to the telegram forms, but in how many cases do the senders affix the stamp themselves? Why, in a very few. At City stations the messages are given in with such quickness that it is almost impossible to know what becomes of your telegram. All you receive for your message is "All right" from the clerk at the counter. What proof is that, especially in evidence in a court of law? None. In this respect, the old system adopted by the various telegraph companies gave infinitely better satisfaction. The acquisition of the telegraphs by the Post-Office was to afford the public better facilities; the question is, have we had them, or are we likely to have them?

There has been a great talk of our having the London telegrams reduced to sixpence, but as yet no notice has been issued to that effect.

Now, I have no doubt the object of the Telegraph Department in refusing to give receipts for telegrams is, because it would consume a quantity of paper and give too much employment to the clerks; but if I may be permitted to offer a few suggestions, I think it could be carried out in this way:—

The counter or receiving clerk should be provided with a rack of small tickets similar to railway tickets, and in the same form, having consecutive numbers, and of various values from one shilling.

A railway date stamp placed on the counter.

These tickets (receipts for telegrams) could be drawn, stamped, and issued to the senders of messages with almost electrical speed.

In cases where it is necessary to produce a copy of a telegram—if it is after twelve months the Telegraph Department destroys them, and as the production of telegrams would often prove a case, it is an important item to men of business. The following suggestion would avoid this:—Senders of telegrams should be supplied, either at the telegraph station or for use at their places of business, with books containing postal telegraph forms, with duplicate forms for copies, similar to those which the various telegraph companies supplied to merchants and others, and by the use of carbonic paper copies could be retained. These copies should be handed, with the original (for transmission), to the receiving clerk, who should affix the date and office-stamp upon the copy. What trouble could that impose on the department? Very little, considering the advantages given to those men who may be said to be the chief supporters of the telegraphs.—I am, &c.,

ARTHUR G. NEWTON.

8, Leadenhall Street, E.C.,
January 14, 1873.

City Notes.

THE close of the year 1872 may be considered as marking an important era in the financial history of telegraphy, viz., the thorough establishment of securities of this kind in the estimation of the public. A further improvement in values has recently taken place, which is, however, to be attributed chiefly to the fact that purchases were stimulated as the season for the payment of the yearly balance dividends drew near. According to Mr. Abbott's circular, the twelve months which have lately ended will be found to have yielded returns of a most satisfactory character, as it is well known that the traffic receipts of the Atlantic lines have attained a very handsome total. What is taking place in the case of these cables is also witnessed on those of the Eastern lines, where the traffic is being carefully fostered and managed. Messages are now transmitted in much less time than it was possible to do a few months ago, and consequently what may be termed the "earning capacity" of the cable continues to increase. It is true that in certain quarters some dissatisfaction has been expressed at the low rate of dividend paid by the Eastern, as compared with some other submarine cables,—for example, the British Indian Extension,—and surprise has been expressed that more capital should be required for a new cable. It is asked why the existing cable only pays about 6 per cent, if the Company is so full of business that an additional one is required. However there really does not appear to be the slightest reason to doubt the prudence of the directors, nor to question the wisdom of the amalgamations which are now taking place. The preliminary arrangements for the amalgamations of the lines east of Bombay seem to be progressing satisfactorily, while it is gratifying to learn that representatives from the various Australian colonies have been deputed to meet in Sydney and fix the amount of subsidy to be paid in exchange for a reduction of tariff. At the same time the public should be warned that cheap telegraphy can never be brought about by competing lines, and those investments should accordingly be shunned which have nothing to recommend them but the hazardous chances of competition. The idea of a sixth Atlantic cable to be laid from Liverpool to New York direct, which has been suggested, is one which we feel inclined to place in this category. The distance is 3200 miles, and the carrying capacity of a capable of this length would be so much less than that of the new French Atlantic one, which is only 800 miles long, that the former would obviously compete on very disadvantageous terms with the latter. Several instances might be adduced from the history of telegraphy of the evils which have resulted from reckless competition, and they all point to the fact that the true course to be pursued is a very different one, viz., for the directors of existing lines to lose no time in completing their arrangements for an amalgamation of all the Submarine Cable Companies into one great organisation, the ramifications of which shall extend over the whole world. By this means expenses can be reduced to a minimum, and many other advantages secured.

Among the chief recent events of importance must be noted the successful introduction of the New York, Newfoundland, and London Telegraph Company. These £20 shares appear to offer a good opportunity for investors who have hitherto held aloof from this class of security. It is confidently asserted that a quarterly dividend will be earned and paid in April next at the rate of at least 10 per cent per annum. This undertaking, possessing as it does exclusive working agreements for 47 years with the Western Union Telegraph Company of the United States, the Anglo-American, and the French Atlantic Companies, is practically secured in its revenue by these three powerful undertakings. It is fair to assume, therefore, that when the question of amalgamation of the Atlantic lines now pending is finally decided upon, the exceptional position occupied by this Company will be favourably considered. Already some extensive exchanges have been made from the shares of the various telegraph companies, showing large profits to holders, into those of the Newfoundland Company. At the settlement, which took place on the 7th inst., in this Company the shares for immediate delivery were extremely scarce. From this it would appear that the demand for investment is considerably in excess of the shares coming to market.

The Telegraph Construction and Maintenance Company have recently adopted a policy which has proved a remarkable success. Owing to the large extension of the business, consequent on the demand for cables bearing their "brand," the Directors, instead of burdening the Company with a permanent addition to the capital, or in other words admitting fresh partners to share the profits of the undertaking, have called upon the proprietors for a temporary loan at a rate of interest satisfactory to both sides; as evidence of this, it may be mentioned that though only £150,000 were offered, no less than 600,000 were applied for, and, further, that the bonds now stand at 1 to 2 premium. In raising money from their Shareholders only, the Directors have opened up a current of support, upon which it is clear they can rely, not only for present requirements, but for any future necessities which may arise in the developing of their splendid business.

An interim dividend of 3s. per share on account of profits, for the three months ending the 31st December last, has been declared by the directors of the China Submarine Telegraph Company.

TELEGRAPH SHARE LIST.

Amount per Share.	Name of Company.	Amount paid up.	Dec. 20th.	Dec. 27th.	Jan. 3rd.	Jan. 10th.
£		£				
Stock	Anglo-American (Limited)	100	129 131	131 133	133 135	132 134
10	British Australian (Limited)	All	9¾ 10	9¾ 10	10⅜ 10⅝	10⅛ 10⅜
10	British Indian Extension (Limited) ..	All	14⅛ 14⅜	14¼ 14¾	14⅝ 14⅞	14¼ 14½ x d
10	China Submarine (Limited)	All	10⅛ 10⅜	10⅜ 10⅝	10⅝ 10⅞	10⅜ 10⅝
10	Cuba (Limited)....	All	8¾ 9¼	8¾ 9¼	9 9½	8½ 9
10	Eastern (Limited)	All	10⅜ 10⅝	10⅝ 10¾	10¾ 11	10⅝ 10⅞
10	Great Northern	All	11 11½	11 11½	11 11½	11 11½
10	Gt. Northern China and Japan Extension	All	— —	— —	— —	— —
10	Hooper's Telegraph Works	All	10¼ 10¾	10¼ 10¾	10¼ 10¾	10¾ 11¼
50	India-Rubber, Gut. Per., and Teleg. Works	45	38½ 39½	35 37	36 38	35 37
25	Indo-European (Limited)	All	14 16	14 16	15 17	15 17
10	Marseilles, Algiers, and Malta..	All	— —	— —	— —	— —
10	Mediterranean Extension (Limited) ..	All	6½ 7	6½ 7	6¾ 7¼	6½ 7
10	Ditto 8 per cent Preference ..	All	11½ 12½	11½ 12½	11½ 12½	11½ 12½
10	Panama and South Pacific (Limited)	2½	— —	— —	— —	— —
8	Reuter's (Limited)	All	9½ 10½	9½ 10½	9½ 10½	9½ 10½
20	Soc. Transatlant. Fran. (Limited)	All	24¾ 25¼	25½ 26	25¾ 26¼	25¾ 26
20	Ditto New	15	— —	— —	— —	— —
Stock	Submarine	100	210 220	210 220	210 220	213 218
1	Ditto Scrip	All	1⅞ 2⅛	1⅞ 2⅛	1⅞ 2⅛	2 2¼
12	Telegrph. Construction and Maintenance	All	31¼ 32	31½ 32	31¼ 31¾	31 31½
10	West India and Panama (Limited) ..	All	6¼ 6½	6 6¼	6⅜ 6⅝	6¼ 6½
1000 dls.	West Un. U.S. 7 per cent 1st M.B. ..	All	— —	— —	— —	— —

There seems to be some contention as to the right of establishing cable communication between the River Plate and Rio de Janeiro. Mr. Basil Holmes, secretary to the Montevidean and Brazilian Telegraph Company, writes to say that, with reference to a statement in the *Times* that a contract has been signed by Messrs. Siemens Brothers to lay a submarine cable between Rio de Janeiro and Montevideo, his company now hold the exclusive concession for a telegraph cable from Montevideo to Brazil, and that the cable will be laid early this year. The final paragraph in the letter, however, seems to show that this concession is only from the Goverment of Uruguay; whereas a joint concession from Brazil and Uruguay will be necessary for the work, and such a concession is fully believed to be in possession of the Platino Braziliera Telegraph Company, with whom Messrs. Siemens are stated to have made a contract.

A map of the world, showing the position of the existing submarine telegraph lines, has been published by Messrs. Bates, Hendy, and Co., of Old Jewry. It has been arranged by Mr. Wm. Abbott, of the Stock Exchange, so as to indicate at a glance the lines now at work and those projected, and will be found very useful by business men.

The Direct Spanish Telegraph Company (Limited) have announced that telegraphic communication with Spain has been established by the laying of a submarine cable between Bilboa, in Spain, and the Lizard, in Cornwall, and that the line was, on December 30th, opened for public traffic.

The traffic returns on the European lines of the Great Northern Telegraph Company for the month of December amounted to 88,785 francs, against 79,483 francs for the corresponding period of 1871, and on the China and Japan lines to 102,810 francs against 59,038 francs.

West Indian Telegraphy.—The cable between Jamaica and Colon is now in working order.

South American Telegraphy.—An important modification has been made in the 10th Article of a concession granted by the Brazilian Government to the Baron de Mana for laying a Transatlantic cable between Brazil and Europe. By a decree of October 30, 1872, the 10th Article now reads as follows: "At the end of twenty years of exclusive privilege the concessionnaire will have the enjoyment of the cable or cables which he may have laid, but without any privilege." Mr. Studdy Leigh has applied to the Chilian Congress for permission to lay down a submarine cable from a point on the Chilian coast.

Patents.

1473. C. H. Siemens, Westminster. *Supports, fastenings, and joints of over ground telegraph wires.* Dated May 15, 1872. This invention relates to modes of constructing the poles for supporting telegraph wires, of fastening the wires to the insulators, securing the insulators in sockets, and joining different lengths of wire together. In constructing the poles of metallic tubes a length is joined to another length, and fixed to the base by making it with a taper end, which is inserted in a taper socket formed on another length or on the base. For fastening the wires to the insulators cams are employed, or double wedge pieces working against rollers, or rollers running in hollows, in such manner that the pull of the wire in either direction moves the cam, wedge piece, or roller, so as to pinch the wire and arrest its movement. Or a cam is made to work across the wire, and is turned by hand so as to bend or pinch the wire, and so hold it fast. The insulators are secured in sockets by forming a projection on the stem of the insulator, passing it through a notch in the socket, and then turning it partly round so as to hold the insulator in the manner of a bayonet catch. The lengths of wires are joined by enclosing the two ends placed end to end in a short tube or casing, and soldering them therein, so that by making the junction dependent on the solder, if electric continuity should be broken by the soldering giving way, the fault is detected by the separation of the wires.

3344. J. B. Stearns, Boston, U.S. *Electric telegraphs.* Dated November 11, 1872. These improvements consist, First, in certain improved modes of constructing the electro-magnets for receiving instruments by means of which the effects of the outgoing electric currents upon the armature of the said electro-magnet or magnets are more perfectly neutralised. Secondly, in an improved arrangement of the rheostats or artificial resistances in combination with the circuit breaker, by means of which the resistance opposed to the battery at each station is always practically the same whatever may be the position of the apparatus at the opposite station. Thirdly, in an improved mode of constructing and operating the circuit breaker or key which transmits the electric currents upon the main line. Fourthly, in the application of a "condenser" or other equivalent device to the apparatus, by means of which the effect of static induction in operating either land lines or submarine cables is neutralised or compensated for. Fifthly, in the manner of placing a relay or other receiving instrument in the circuit of a "bridge-wire," so that the said relay or other receiving instrument will be operated by received currents and not by transmitted currents. Sixthly, in a repeater or translator for use in telegraphic apparatus for double transmission. Seventhly, in an arrangement of circuits whereby one or more branch offices are enabled to operate the transmitting key or circuit breaker at the main office, and also whereby the receiving relay at the main office is caused to operate a receiving instrument at said branch office or offices for the purpose of establishing direct communication between the branch offices in one place and the main office or branch offices in one or more places, or *vice versa*. Eightly, in an arrangement of telegraphic apparatus whereby way stations are enabled to communicate with either or both terminal stations employing duplex instruments or double transmitters.

New Electrical Batteries.—M. Lionel Weber has invented a new battery composed of a porous diaphragm filled with plumbago. This vessel is placed in a glass or porcelain vase containing a saturated solution of ammoniacal chlorhydrate. Into the plumbago is introduced a plate of charcoal, which constitues the positive pole; and into the solution which surrounds the diaphragm is plunged a plate of amalgamated zinc, forming the negative pole. This battery has been found to have great force, to be constant in the support of regular and continued work, to be economical, and to need but little attention.

A new galvanic pile, invented by M. Morin, is intended to avoid the inconvenience caused by the deposit of copper upon the surface of the zinc or upon the porous cup. The pile consists of a cylinder of copper surrounded by a concentric cylinder of zinc, between which two cylinders is a third cylinder of filtering paper. There is difference enough in the size of these cylinders to leave concentric angular spaces between the paper and the copper and the paper and the zinc, The former space is filled with sand, and the latter with a stratum of flowers of sulphur. The whole is immersed in sulphate of copper. Such a pile, it is said, has operated during five months with so little variation that the inventor believes it would equally work well for an additional five. During these five months the current has been continuous.

The *Asiatic* says:—"We have heard of the disagreeable hitch that seems to occur periodically in the money-order system between England and India, and now it appears that a conference has been held at Simla on the subject of amalgamating the Indian Telegraph and Postal Department. We understand that Mr. Monteith has already submitted to Government a report, in which, while admitting the utility of the measure so far as expenditure is concerned, he has observed that the success of the scheme in England furnishes no guarantee of its success in India, the circumstances of the two countries being quite dissimilar."

Quick Telegraphing.—The President's Message reached the telegraph office in Washington at twenty-five minutes past one o'clock. Within forty-five minutes, the shortest time ever attained with so long a document, it was transmitted to New York, Philadelphia, and Baltimore. Twelve wires were used, and the weather was favourable for working purposes. There were 11,339 words in the message. Within four hours after leaving Washington it was published in full in the evening papers and sold about the streets in this city.

It has been suggested in America, that telegraph wires be extended to all the lighthouses on the coast, and that a system of signals be arranged to be exhibited from the lighthouses to give notice to passing vessels of approaching storms or changes of wind.

Dr. Tyndall in America,—At a lecture at Boston, Professor Tyndall said:—"There are three great theories which enable the human mind to open the secrets of nature—the theory of gravitation, the mechanical theory of heat, and the undulatory theory of light. These three pillars, as far as the human intellect in concerned, support the universe. To whom are we indebted for these discoveries? To men who had no practical ends in view, and who cared only for the truth. To-day, when there are so many temptations to young men to leave pure science for practical aims, it behoves us to look with sympathetic eyes upon the investigator who makes all this knowledge possible. I met on the steamship *Russia* a respected friend who ascribed the electric telegraph to a source to which I certainly should not have thought of referring it. It is the direct outcome of men who never made a shilling by it. Volta, Faraday, never made a shilling by it. All honour to the men who made these discoveries. Gauss and Weber, at the University of Göttingen, actually constructed a telegraph line from the physical cabinet to the observatory. Give all honour to the men who apply discoveries, but do not forget the men who make them. Many of you in this country have made fortunes, and have shown that you know how to apply them. Look with sympathetic eyes upon the investigators. Give them opportunities. Do not overload them with other work. 'Cast your bread upon the waters,' and believe me 'it will return to you after many days.' My course among you is nearly over. I began it with some anxiety, and end it with regret. It has been harder for me at times than I had expected, and I owe much to my assistants. I shall long and gratefully remember my reception on the occasion of my first lecture here. If I am treated in the same manner elsewhere, I shall return to the old country full of content. During my stay here I have heard 'the old country' mentioned again and again. You cannot abolish your antecedents. Out of England's loins you have come. Your ancestry is stamped upon your faces, your laws, your politics, and your characters, De Tocqueville, sympathising with democratic institutions, says, regarding England and America: 'I refuse to regard these people as two; one is the outgrowth of the other.' Atrocious ignorance of each other is at the bottom of all our differences. I trust that hereafter each nation will respect the individuality of the other while thoroughly maintaining its own.

During the experiments by the Torpedo and Gun-cotton Committees, a discovery of considerable importance was made by Mr. E. O. Brown, assistant chemist to the War Department—viz., that a charge of gun-cotton saturated with water can be fired by electricity with a detonating fuze. The result of the several experiments may be briefly summarised as follows:—That under the conditions tried, nitrated gun-cotton possesses slightly more power than ordinary gun-cotton; that picric powder is very nearly, if not quite, equal to gun-cotton in explosive power; that both gun-cotton and picric powder are far superior to gunpowder, that damp or saturated gun-cotton can be exploded by means of a detonating fuze, with but little loss of power as compared to dry cotton.

Societies' Meetings for the Month.

Meteorological Society.—Ordinary and Council Meeting, Jan. 15, 1873.
Geological Society.—Jan. 22.—Feb. 5.
Society of Telegraph Engineers.—Jan. 22.—Feb. 12.

To Correspondents.

We shall next month present to our students a simple account of Professor Sir William Thomson's principal electrical inventions.